JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

MAJOR FRANK BOLTON, HON. SECRETARY,

AND

GEO. E. PREECE, SECRETARY.

VOL. II.—1873.

TABLE OF CONTENTS.

Vol. II.

CONTENTS.

LIST OF MEMBERS

OF

THE SOCIETY

OF

TELEGRAPH ENGINEERS.

ESTABLISHED 1871.

CORRECTED TO 31st DECEMBER, 1873.

LONDON:
NICHOLS AND SONS, 25, PARLIAMENT STREET, WESTMINSTER.

1874.

NOTICE.

It is particularly requested that any change of Residence or Address may be communicated at once to the Secretary.

The Society of Telegraph Engineers.

ESTABLISHED 1871.

COUNCIL.

President.

SIR WILLIAM THOMSON, F.R.S., LL.D.

Past-Presidents.

FRANK IVES SCUDAMORE, C.B.
CHARLES WILLIAM SIEMENS, F.R.S., D.C.L., M.INST.C.E.

Vice-Presidents.

THE LORD LINDSAY.
LATIMER CLARK, M.INST.C.E.
R. S. CULLEY, M.INST.C.E.
PROFESSOR G. C. FOSTER, F.R.S.

Members.

PROFESSOR ABEL, F.R.S.
MAJOR MALCOLM, R.E.
W. H. PREECE, M.Inst.C.E.
ROBERT SABINE, C.E.
CARL SIEMENS, M.Inst.C.E.
C. E. SPAGNOLETTI.
LIEUT.-COLONEL STOTHERD, R.E.
C. V. WALKER, F.R.S.
MAJOR C. E. WEBBER, R.E.
WILDMAN WHITEHOUSE.
PROFESSOR WILLIAMSON, F.R.S.
CROMWELL VARLEY, F.R.S., M.Inst.C.E.

Associates.

ANDREW BELL.
DR. A. MUIRHEAD.
LIEUT. WATSON, R.E.

OFFICERS.

Auditors.

J. WAGSTAFF BLUNDELL.
FREDERICK C. DANVERS (India Office).

Hon. Treasurer.

MAJOR C. E. WEBBER, R.E.
101, Cannon Street, E.C.

Hon. Secretary.

MAJOR FRANK BOLTON.
4, BROAD SANCTUARY, S.W.

Hon. Solicitors.

Messrs. WILSON, BRISTOWS, & CARPMAEL.

Secretary.

GEO. E. PREECE, 31, Bedford Street, Covent Garden, W.C.

Office.

4, BROAD SANCTUARY, LONDON, S.W.

LOCAL HONORARY SECRETARIES.

W. E. Ayrton, Professor of Natural Philosophy, Imperial College, Yokohama, Japan	JAPAN.
Charles Burton, Director-General of the Argentine Telegraphs, Buenos Ayres	ARGENTINE REPUBLIC.
Le Commandeur E. D'Amico, Director-General of the Italian Telegraphs, Rome	ITALY.
Frédéric Delarge, Engineer of the Belgian Telegraphs, Brussels	BELGIUM.
C. L. Madsen, Great Northern Telegraph Company, Copenhagen	DENMARK.
C. Nielsen, Director-General of the Norwegian Telegraphs, Christiania	NORWAY.
Don Ramon Pias Director-General of the Chilian Telegraphs, Santiago	CHILI.

Society of Telegraph Engineers.

LIST OF MEMBERS,

CORRECTED UP TO 31st DECEMBER, 1873.

HONORARY MEMBERS.

SIR GEORGE BIDDER AIRY, K.C.B., D.C.L., LL.D., *Astronomer Royal.*	Greenwich.
GEN. SIR EDWARD SABINE, R.A., K.C.B., D.C.L., LL.D.	13, Ashley Place, Westminster, S.W.
PROFESSOR WILHELM WEBER, F.R.S.	Göttingen.

FOREIGN MEMBERS.

AGUAYO, DON WALDO . . .	Santiago, Chili.
AILHAUD, M.	Inspector-General of the French Telegraphs, Paris.
ALTENECK, F. VON HEFNER . .	94, Markgrafen Strasse, Berlin.
ARAUJO, HIPPOLYTE . . .	Inspector of Spanish Telegraphs, Madrid.
BATES, D. H.	Western Union Telegraph Company, Philadelphia.
BRÉGUET, ALFRED NIAUDET . .	39, Quai de l'Horloge, Paris.
BRÉGUET, M. SEN. . . .	39, Quai de l'Horloge, Paris.
BROOKS, DAVID	22, South Twenty-first Street, Philadelphia, U.S.
BROWN, A. S.	Western Union Telegraph Company, New York.
BURTON, CHARLES . . . (*Honorary Secretary for the Argentine Republic.*)	Director-General of the Argentine Telegraphs, Buenos Ayres.

Calahan, E. A.	17, Cornhill, E.C.
Capanena, Professor	Director-General of the Brazilian Telegraphs.
Chandler, Albert B.	Western Union Telegraph Company, New York.
Dakers, James	Manager, Montreal Telegraph Company, Montreal, Canada.
D'AMICO, M. le Commandeur, E. (*Honorary Secretary for Italy.*)	Director-General of the Italian Telegraphs, Rome.
DELARGE, FRÉDÉRIC (*Honorary Secretary for Belgium.*)	Engineer of the Belgian Telegraphs, Brussels.
Dolan, T.	Western Union Telegraph Company, New York.
Dwight, J. H.	Western Union Telegraph Company, New York.
Eckert, Gen. Thomas	Western Union Telegraph Company, New York, U.S.
Field, Cyrus	New York, U.S.
Frischen, Carl	94, Markgrafen Strasse, Berlin.
Girardin, Joseph	Director of Belgian Telegraphs, Brussels.
Guilleaume, F. C.	Cologne, Rhenish Prussia.
Haskins, C. H.	North Western Telegraph Company, Milwaukee, Wisconsin, U.S.
Hoffmier, Gustav	Great Northern Telegraph Company, Shanghai.
Hoskiar, Capt. V.	Royal Danish Engineers, Copenhagen.
Hughes, Professor, D. E.	85, Rue Notre Dame des Champs, Paris.
Kellogg, Milo G.	220, East Kinzie Street, Chicago, U.S.
Lacoine, Emile	Director of Telegraphs, Constantinople.
Ladd, George J.	Western Union Telegraph Company, San Francisco, U.S.
Lasard, Dr. Ad.	6, Hohenzollern Strasse, Berlin.
Lasard, Dr. Adolphe	German Union Telegraph Company, Werder Strasse, Berlin.
Lüders, His Excellency Gen. von	Director-General of the Imperial Russian Telegraphs, St. Petersburgh.

MADSEN, C. L. . . . . (*Honorary Secretary for Denmark.*)	Great Northern Telegraph Company Copenhagen.
MARTIAL, DE SAINT . . .	Secretary of the International Bureau.
MERRIHEW, J.	Western Union Telegraph Company, Philadelphia, U.S.
MEYDAM, Colonel T. . . .	Director-General of the Imperial German Telegraphs, Berlin.
MILLIKEN, G. F. . . .	Western Union Telegraph Company, Boston, U.S.
NIELSEN, C. . . . . (*Honorary Secretary for Norway.*)	Director-General of the Norwegian Telegraphs, Christiania.
ORTON, Hon. WILLIAM . . .	President, Western Union Telegraph Company, 145, Broadway, New York.
PIAS, DON RAMON . . . (*Honorary Secretary for Chili.*)	Santiago, Chili.
POPE, FRANK L. . . .	Elizabeth, New Jersey, U.S.
PRESCOTT, GEORGE . . .	Western Union Telegraph Company, New York, U.S.
REPSOLD, J. G.	Rio Janeiro, Brazil.
SARGENT, W. D. . . .	814, Race Street, Philadelphia, U.S.
SALVATORI, F.	Inspector-in-Chief of the Italian Telegraphs, Rome.
SIEMENS, Dr. WERNER . . .	Berlin.
STARING, M.	Director of the Netherland Telegraphs, Hague.
STEARNS, J. B.	
SUMMERS, C. H. . . .	Western Union Telegraph Company, Chicago, Illinois, U.S.
TILLOTSON, L. G. . . .	8, Dey Street, New York, U.S.
TORRES, DON DIEGO . . .	Santiago, Chili.
TRANT, LAWRENCE B. . . .	Argentine Telegraph Department, 103, Calle Cangallo, Buenos Ayres.
TRANT, PETER NICHOLAS . .	Argentine Telegraph Department, Rosario, Argentine Republic.
UGARTO, DON A. V. . . .	Santiago, Chili.
VINCHENT, M.	Inspector-General of the Belgium Public Works, Brussels.
ZETZSCHE, Dr. K. E. . . .	Chemnitz, Saxony.

Total number of Foreign Members . . 57

MEMBERS.

ABEL, PROFESSOR, F.R.S. (*Member of Council.*)	Royal Arsenal, Woolwich.
ABNEY, W. Capt., R.E.	Chatham.
ADAMS, W. G. Professor	Physical Laboratory, King's College, W.C.
ANDERSON, S. Capt., R.E.	British N. America (Messrs. Cox and Co., London).
ANDREWS, THOMAS	French Atlantic Telegraph Company, Brest.
ANDREWS, W. S.	16, Telegraph Street, E.C.
ANSELL, WILLIAM T.	66, Old Broad Street, E.C.
ARMSTRONG, R. Y., Capt., R.E.	Chatham.
ASHURST, W. H.	General Post Office, E.C.
ATKINSON, E., Dr., F.C.S.	8, The Terrace, York Town, Surrey.
AYLMER, JOHN	4, Rue de Naples, Paris.
AYRTON, W. E. (*Honorary Secretary for Japan.*)	Imperial College, Yokohama, Japan.
BAILEY, W. H.	Brighton View, Leith Square, Pendleton, Manchester.
BAKER, VALENTINE, Col. 10th Hussars	13, St. James's Place, S.W.
BARKER, G., Lieut. R.E.	Bermuda.
BARLOW, WILLIAM HENRY, F.R.S.	2, Old Palace Yard, S.W.
BATEMAN-CHAMPAIN, Major, R.E.	55, Parliament Street, S.W.
BEAUMONT, F. Major, R.E.	3A, Victoria Street, Westminster, S.W.
BECKER, CHAS.	112, St. Martin's Lane, W.C.
BOLTON, FRANK, MAJOR (*Honorary Secretary.*)	4, Broad Sanctuary, Westminster, S.W.
BRASHER, A.	55, Parliament Street, S.W.
BRIGHT, SIR CHARLES, M.I.C.E.	26, Duke Street, Westminster, S.W.
BRISTOW, G. L. (*Honorary Solicitor.*)	1, Copthall Buildings, E.C.
BROADBENT, T. E., Lieut., R.E.	Calcutta, India.
BROUGH, S.	Indian Government Telegraphs, Calcutta, India.
BROWN, E. O.	Royal Arsenal, Woolwich, S.E.
BURKE, E. HAVILAND, M.P.	54, Brompton Crescent, S.W.
BURKE, J.	Indian Government Telegraphs.
BURT, J. T.	19, Conningham Road, W.
CANNING, Sir SAMUEL, C.E.	7, Great Winchester Street Buildings, Old Broad Street, E.C.
CARGILL, W. W., F.R.G.S.	Director-General of Imperial Telegraphs, Yokohama, Japan.
CECIL, Lord SACKVILLE	Hayes Common, Beckenham, Kent.

CHAMBRE, ALAN E.	Camera Lodge, South Norwood, S.E.
CHAUVIN, GEORGE VON	48, Palmerston Buildings, Old Broad Street, E.C.
CLARK, EDWIN, M.I.C.E.	5, Westminster Chambers, Westminster, S.W.
CLARK, LATIMER, M.I.C.E. (*Vice-President.*)	5, Westminster Chambers, Westminster, S.W.
COLLETT, RICHARD	25, Burghley Road, Highgate Road, N.W.
COLOMB, P.H. Capt. R.N.	Roxeth Villa, Harrow-on-the-Hill.
COOKE, Sir W. FOTHERGILL	Branksea Lodge, Tooting, S.W.
CRACKNELL, E. C.	Sydney, Australia.
CRACKNELL, W. F.	Brisbane, Australia.
CRIPPS, EDWARD	London Brighton and South Coast Railway, London Bridge.
CROCKER, HENRY RING	15, Kent Terrace, East Greenwich.
CULLEY, R. S., M.I.C.E. (*Vice-President.*)	Postal Telegraphs, General Post Office.
DANVERS, JULAND	India Office, S.W.
DEANE, THOMAS	Common Side, Mitcham, Surrey.
DEN, JOHN D.	Telegraph Department, Cape Town, Cape of Good Hope.
DORMAN, MARK, J.P.	Melbourne Crescent, Northampton.
DOUGLAS, Col.	East India United Service Club, 14, St. James's Square, S.W.
ECKFORD, J. Capt. R.E.	Calcutta, India.
ELLIS, WILLIAM, F.R.A.S.	Royal Observatory, Greenwich.
ERICHSEN, H. G.	7, Great Winchester Street Buildings, E.C.
EVANS, MORTIMER	West Regent Street, Glasgow.
FEATHERSTONHAUGH, A. Capt. R.E.	British N. America (Messrs. Cox & Co., London).
FFINCH, B. T.	Calcutta, India.
FITZGERALD, D. G.	6, Loughborough Road, North Brixton, S.W.
FLOYD, WILLIAM HENRY	Government Telegraph Office, Wellington, New Zealand.
FORDE, H. C., M.I.C.E.	6, Duke Street, Adelphi, W.C.
FOSTER, G. C., PROFESSOR, F.R.S. (*Vice-President.*)	12, Hilldrop Road, N.
FRANCE, J. R.	27, Amherst Road, Hackney.
FRANCIS, FRANK RICHARD	125, New Kent Road, S.E.
FRASI, FREDERICK	65, Brewer Street, Woolwich, S.E.
FULLER, THOMAS	119, Glo'ster Terrace, Hyde Park.

Galton, Douglas, Capt., C.B., F.R.S.	Chester Street, Grosvenor Place, S.W.
Gebhardi, Charles A.	153, Queen's Road, Dublin.
Gill, W. J., Lieut. R.E.	Aldershot.
Gilmore, A. H., Commander R.N.	Junior Carlton Club, Pall Mall, S.W.
Gladstone, Dr. John Hall, F.R.S.	17, Pembridge Square, W.
Glover, T. G., Col. R.E.	Burwood, Hersham, near Esher.
Goodeve, T. M., Professor	Goldsmith Buildings, Temple, E.C.
Goldstone, Charles	London and South Western Railway, Southampton.
Gordon, C. P. B.	Indian Government Telegraphs.
Graves, A.	North Eastern Railway, 46, Monkgate, York.
Graves, E.	Postal Telegraphs, Birmingham.
Graves, James	Anglo-American Telegraph Company, Valentia, Ireland.
Gray, Matthew	106, Cannon Street, E.C.
Greener, J. H.	84, Lombard Street, E.C.
Greenhill, Matthew C.	Postal Telegraphs, Belfast.
Griffith, G., Master of Harrow School	Harrow.
Grimston, George Sylvester	Crane Hall, Ipswich, Suffolk.
Grover, George E., Capt. R.E.	Halifax, Nova Scotia.
Gutteres, J.	Kingston, Jamaica.

Hall, William Henry, Lieut. R.N.	Byne Lodge, Stonington, Sussex.
Halpin, Captain Robert	38, Old Broad Street, E.C.
Hawkins, Frederick	Silvertown.
Haynes, Frederick T. Iago	Bristol and Exeter Railway, Taunton.
Henley W. T.	Chesterton House, Plaistow, Essex.
Hockin, Charles	8, Avenue Road, St. John's Wood.
Holmes, Nathaniel John	7, Great Winchester Street Buildings, Old Broad Street, E.C.
Home, R., Capt. R.E.	Topographical Department, War Office, New Street, Spring Gardens, S.W.
Hooper, J. P.	"The Hut," Mitcham.
Hyde, H., Col. R.E.	Calcutta, India.

James, F. R.	Government Telegraph Department, Melbourne, Australia.
Jekyll, H., Lieut. R.E.	Postal Telegraphs, 101, Cannon Street, E.C.
Jenkin, Fleeming, F.R.S., M.I.C.E.	3, Great Stuart Street, Edinburgh.
Jervois, W. H., Col. R.E., C.B.	War Office, S.W.
Johnson, J. Thewlis	27, Dale Street, Manchester.
Johnstone, W. P.	Indian Government Telegraphs.

LAMBERT, FRANK	15, Great Castle Street, Regent's Circus.
LAMBERT, M., Capt. R.E.	Barbadoes (Messrs. Cox & Co., London).
LAUCKERT, EDWARD	5, Lower Woodland Place, New Charlton, S.E.
LANGDON, WILLIAM	Postal Telegraphs, Southampton.
LAWS, J. C.	Telegraph Construction and Maintenance Company, 38, Old Street, E.C.
LEMON, C.	Wellington, New Zealand.
LE MESURIER, T. A., Capt. R.E.	Chatham.
LINDSAY, LORD *(Vice-President.)*	47, Brook Street, W.
LOEFFLER, LOUIS	3, Great George Street, Westminster, S.W.
LUKE, S. P.	Calcutta, India.
MAITLAND, JAMES M. H. Major R.E.	Mount Wise, Devonport.
MALCOLM, E. D. Major R.E. *(Member of Council.)*	Chatham.
MALLOCK, Capt.	Indian Government Telegraphs, Simla.
MAXWELL, Professor Clerk, F.R.S.	Cambridge.
McGOWAN, SAMUEL	Telegraph Department, Melbourne.
MITCHEL, SIR WILLIAM	54, Gracechurch Street, E.C.
MITTELHAUSEN, JULIUS	40, Upper Maryon Road, New Charlton, S.E.
MONCRIEFF, Major ALEXANDER	Athenæum Club, S.W.
MOXON, HENRY	Lancashire and Yorkshire Railway, Manchester.
MUIRHEAD, JOHN	159, Camden Road Crescent, N.W.
MUIRHEAD, JOHN, Junior	(W. M. Warden & Co.'s) Carey Street, Westminster, S.W.
NAGLO, EMIL	44, Waldemar Strasse, Berlin.
NEWMAN, G. G.	London and North Western Railway, London Road, Manchester.
ORMISTON, FREDERICK A.	
PARSONS, Lieut.-Col. R.M., F.R.S.	Ordnance Survey Office, Southampton.
PEPPER, J. H., Professor	
PHILLIPS, SAMUEL E.	8, Stretton Villas, South Hackney, E.
PHILLIPS, SAMUEL E. Junior	Fern Cottage, Upper Whitworth Road, Plumstead.

PLAYFAIR, Dr. LYON, C.B., LL.D., F.R.S.	4, Queensbury Place, South Kensington, W.
POSSMANN, J.	Indo-European Telegraph Department, Persian Gulf.
PREECE, JOHN RICHARD . .	Indo-European Government Telegraphs, Ispahan, Persia.
PREECE, G. E. . . . . *(Secretary.)*	31, Bedford Street, Covent Garden, W.C.
PREECE, W. H., M.I.C.E. . . *(Member of Council.)*	Grosvenor House, Southampton.
RADCLIFFE, ARTHUR . . .	7, Union Street, Birmingham.
RADCLIFFE, JAMES . . .	Great Northern Railway, Retford.
REUTER, BARON DE . . .	Kensington Palace Gardens.
REYNOLDS, CHARLES . . .	Calcutta, India.
RICKETTS, F. H.	3, Great George Street, Westminster, S.W.
ROBINSON, D., Col. R.E. . . .	Director-General of Indian Telegraphs, Calcutta, and Rectory, Catsfield, Hawkhurst, Sussex.
ROSENBUSCH, EDWARD . . .	Mediterranean Extension Telegraph Company, Valletta, Malta.
SABINE, ROBERT, C.E. . . *(Member of Council.)*	Auckland House, Willesden Lane, N.W.
SACH, HENRY	Great Eastern Railway, Shoreditch Station, E.
SANGER, THOMAS HENRY . .	Postal Telegraphs, Dublin.
SAUTY, C. V. DE . . .	Eastern Telegraph Company, Gibraltar.
SAUNDERS, JOHN BREWER . .	The Laurels, Taunton.
SCHAW, Lieut.-Col. HENRY, R.E. .	Staff College, Farnboro' Station.
SCHWENDLER, L., C.E. . . .	Indian Government Telegraphs, Calcutta, India.
SCOTT, R. G. Lieutenant R.E. . .	Chatham.
SCUDAMORE, FRANK IVES, C.B. *(Past-President.)*	General Post Office, St. Martin's-le-Grand, E.C.
SHAW, W. H.	Postal Telegraphs, Newark, Notts.
SIEMENS, CARL, M.I.C.E. . . *(Member of Council.)*	3, Great George Street, Westminster, S.W.
SIEMENS, CHARLES WILLIAM, F.R.S., D.C.L. *(Past President.)*	3, Great George Street, Westminster, S.W.
SILVER, S. W.	67, Cornhill, E.C.
SIMMONS, Sir LINTORN, Major-Gen. R.E., K.C.B.	Royal Military Academy, Woolwich.
SMITH, THEOPHILUS . . .	6, Merrick Square, Borough, S.E.
SMITH, WILLOUGHBY . . .	18, Wharf Road, City Road, N.
SPAGNOLETTI, C. E. . . . *(Member of Council.)*	G. W. Railway, Paddington, W.

SPRAGUE, J. T.	Birmingham.
SPROT, J. Lieut.-Col. . . .	Junior United Service Club, Waterloo Place.
STOCKLEY, Major, R.E. . . .	Chatham.
STOTHERD, R. H. LIEUT.-COLONEL R.E. *(Member of Council.)*	War Office, Whitehall, S.W.
TANSLEY, EDWARD . . .	Postal Telegraphs, Edinburgh.
TAYLOR, HERBERT . . .	The Avenue, Gipsy Hill, S.E.
TEALE, FRED. G. . . .	Indian Government Telegraphs, care of Grindlay and Co. 55, Parliament Street, S.W.
TERNANT, A. L.	Eastern Telegraph Company, Marseilles, France.
THOMSON, SIR WILLIAM, F.R.S.., LL.D. *(President.)*	The University, Glasgow, N.B.
TIETGEN, C. F.	Great Northern Telegraph Company, Copenhagen.
TODD, CHARLES, C.M.G. . .	Director General South Australian Telegraphs, Adelaide, Australia.
TODD, Lieut. R.E. . . .	Chatham.
TREUENFELD, G., FISCHER VON .	3, Great George Street, Westminster, S.W.
TURNER, H. T., Capt. R.E. . .	Postal Telegraphs, Ipswich.
TYNDALL, Professor, JOHN, LL.D., F.R.S.	Royal Institution, Albemarle Street, W.
VARLEY, C. F., F.R.S., M.I.C.E. *(Member of Council.)* .	1, Great Winchester Street Buildings, Old Broad Street, E.C.
VARLEY, F.H.	Mildmay Park Works, Stoke Newington, N.
VARLEY, S. A.	66, Roman Road, Holloway, N.
WALKER, C. V., F.R.S. *(Member of Council.)* . .	Fern Side, Red Hill, Reigate.
WALKER, E. J. DOUGLAS . .	Indo-European Telegraphs, Persia.
WALSH, JOHN	Postal Telegraphs, Manchester.
WARDEN, W. M. . . .	Broad Street, Birmingham.
WARREN, T. T. P. B. . . .	Tamworth House, Mitcham, Surrey.
WEAVER, HENRY . . .	1, Albert Square, Clapham Road, S.W.
WEBB, F. C., M.I.C.E. . . .	57, Maitland Park Road, N.W.
WEBBER, C. EDMOND, Major R.E. *(Member of Council.)*	Postal Telegraphs, 101, Cannon Street, E.C.
WHEATSTONE, Sir CHARLES, D.C.L., F.R.S.	19, Park Crescent, Portland Place, N.W.

WHITEHOUSE, E. O. WILDMAN *(Member of Council.)*	Hampstead, N.W.
WILLIAMSON, PROFESSOR . . *(Member of Council.)*	University College, Gower Street, W.C.
WINTER, G. K., F.R.A.S. . .	Arconum, near Madras.
WINTER, WILLIAM HENRY . .	Postal Telegraphs, General Post Office.

Total Number of Members . . . 185

ASSOCIATES.

Adams, A. I. S.	Postal Telegraphs, General Post Office.
Adamson, E. W.	Submarine Telegraph Company, Eastdean, Eastbourne.
Aird, John, Junr.	Belvedere Road, Lambeth.
Allen, J. J.	Indian Government Telegraphs, Calcutta.
Allen, James	Telegraph Superintendent, Great Southern Railway, Buenos Ayres.
Angell, Captain Thomas	Buckingham Gate, S.W.
Arnold, George	Postal Telegraphs, G.P.O., E.C.
Arnold, John	Postal Telegraphs, G.P.O., E.C.
Ascough, T. B.	Postal Telegraphs, 101, Cannon Street, E.C.
Ashton, Edwin	Postal Telegraphs, Camden Town, N.W.
Bailey, John	India Rubber Co., Silvertown, E.
Bain, Alexander	
Banker, S. M.	Postal Telegraphs, G.P.O., E.C.
Barrow, Captain Robert Knapp	94, Piccadilly, W.
Bayley, E. W.	Postal Telegraphs, Hull.
Bazeley, Thomas	Postal Telegraphs, Cardiff.
Beckett, J. W.	Postal Telegraphs, Lancaster.
Beckinsale, Edgar William	23, Langley Street, Newport, Isle of Wight.
Belchamber, W.	Postal Telegraphs, Cardiff.
BELL, ANDREW *(Member of Council.)*	Postal Telegraphs, Gloucester Road, N.W.
Bell, Alexander Carlyle	7, Great Winchester Street Buildings, E.C.
Ben'est, Henry	India Rubber Company, Silvertown.
Bent, William	6, Duke Street, Adelphi, W.C.
Betts, A. S.	Kurrachee.
Betts, James A.	106, Cannon Street, E.C.
Bewick, T. J., C.E.	4, Broad Sanctuary, Westminster, S.W.
Biddulph, W. W.	Care of Messrs. Grindlay and Co., 55, Parliament Street.
Black, Francis Daniel	Postal Telegraphs, Glasgow.
Blewett, George	Postal Telegraphs, Oxford.
Blissett, T.	Calcutta.
BLUNDELL, J. W. *(Auditor.)*	16, Gresham Street, E.C.

BOHR, HEINRICK . . .	Great Northern Telegraph Company, Amoy, China.
BOTELER, R.	Indian Government Telegraphs, Calcutta.
BOTTOMLEY, J. T. . . .	The College, Glasgow, N.B.
BOTTOMLEY, WILLIAM . . .	3, Collyer's Buildings, Blackheath Hill.
BORDEAUX, JOHN . . .	Submarine Telegraph Company, Dover.
BROWN, R. T.	West India and Panama Telegraph Company, Kingston, Jamaica.
BROWN, F. R.	Indian Government Telegraphs, Calcutta.
BROGDEN. JAMES . . .	5, Queen's Square, Westminster.
BUCKNILL, J. T., Lieut. R.E. . .	War Office, Whitehall, S.W.
BULLIVANT, W. M. . . .	59, Fenchurch Street, E.C.
BULMER, J. A.	Postal Telegraphs, Hull.
CAMPBELL, HENRY . . .	9, Hammer's Terrace, Greenwich, S.E.
CARLISLE, FRANK. W. . . .	Charlton, S.E.
CARPENTER, F. H. . . .	Western and Brazilian Telegraph Company, Para.
CARR, GEORGE M. . . .	Postal Telegraphs, Sunderland.
CAWOOD, REUBEN . . .	Postal Telegraphs, Glasgow, N.B.
CHAMBERS, JOHN CHARLES . .	Postal Telegraphs, Stockton-on-Tees.
CHAMBRE, CHARLES F. . . .	Manor Cottage, Putney.
CHANDLER, ROBERT HARVEY . .	106A, Jermyn Street, S.W.
CHARLES, G. G.	Indian Government Telegraphs.
CLAPP, W. H.	14A, Austin Friars, E.C.
COLLINGS, CHARLES E. . . .	Postal Telegraphs, Plymouth.
COLLINS, A.	4, Broad Sanctuary, Westminster, S.W.
COLLINS, LAWRENCE . . .	Postal Telegraphs, 88, South Mall, Cork.
COLLINS, W. C.	24, Old Jewry, E.C.
COMPORT, GEORGE HENRY . .	Postal Telegraphs, Nottingham.
COOK, E. E.	Postal Telegraphs, 101, Cannon Street, E.C.
COOKSLEY, W.	Postal Telegraphs, Bristol.
CORNER, C. R.	Eastern Telegraph Company, Vigo, Spain.
COX, H. J.	48, Arthur Road, Holloway, N.
CRACE, H. WINFIELD . . .	31, Lombard Street, E.C.
CRAMPTON, T. RUSSELL, M.I.C.E. .	4, Victoria Chambers.
CROMARTIE, DUNCAN . . .	Indian Government Telegraphs, Calcutta.
CROSS, P. W. . . .	Postal Telegraphs, Cardiff.
CROSSLEY, LOUIS J. . .	Moorside, Halifax, Yorkshire.
CUNBILL, FRANK W. . .	Postal Telegraphs, Plymouth.

DALLAS, JOHN . . .	. Telegraph Construction and Maintenance Company, Morden Wharf, Greenwich.
DANIELL, FRANCIS T. BRISTOW	. Indo-European Government Telegraphs, 55, Parliament Street, S.W.
DANVERS, F. C. (*Auditor.*) . .	. India Office, S.W.
DAVIES, WILLIAM HENRY	. Exchange Telegraph Company, 17, Cornhill, E.C.
DAVY, HENRY . . .	. 2, Lime Terrace, New Charlton, S.E.
DELANY, PATRICK . .	. Postal Telegraphs, 52, Ranelagh Road, Dublin.
DENMEAD, THOMAS . .	. Postal Telegraphs, Exeter.
DIXON, G. H. . . .	. Postal Telegraphs, General Post Office.
DOHERTY, JOHN . .	. Postal Telegraphs, Manchester.
DONOVAN, H. C. . .	. 44, New Manor Road, New Cross, S.E.
DORMAN, THOMAS . .	. Postal Telegraphs, Stockton-on-Tees.
DOUGLAS, JOHN . .	. Indian Government Telegraphs.
DRURY, WILLIAM, Sen. . .	. 61, South Hill Park, Hampstead.
DUNN, ANDREW S. . .	. Telegraph Department, Caledonian Railway Co., Glasgow, N.B.
DURRAN, WILLIAM . .	. Eastern Telegraph Company, Alexandria.
EDEN, AUGUSTUS . .	. Postal Telegraphs, Edinburgh.
EDWARDS, J. R. . . .	. Postal Telegraphs, Chester.
EGGINGTON, ALFRED . .	. Suza-Modica Telegraph, Florence, Italy.
ELLIOTT, THOMAS . .	. Postal Telegraphs, Basingstoke.
EVANS, FREDERICK E. . .	. Postal Telegraphs, Birmingham.
FAHIE, J. J. . . .	. Indo-European Telegraphs, Jask, Persian Gulf.
FALCK, A. . . .	. Great Northern Telegraph Company, Aberdeen, N.B.
FAULKNER, JOHN . .	. 13, Great Ducie Street, Strangeways, Manchester.
FIELD, GEORGE . .	. Postal Telegraphs, Old Steine, Brighton.
FISHER, HENRY . . .	. Postal Telegraphs, General Post Office, E.C.
FISHER, HENRY F. . .	. British Indian Extension Telegraph Company, Singapore.
FISHER, J. A., Commander R.N.	. H.M.S. "Excellent," Portsmouth.
FLEETWOOD, CHARLES THOMAS	. Post Office Telegraphs, General Post Office, E.C.

FLEMING, J. C.	Supt. Government Telegraphs, Perth, Western Australia.
FLETCHER, JOHN WILLIAM	London and North Western Railway Telegraph Department, Chester.
FLETCHER, THOMAS	Hooper's Telegraph Works, Millwall, E.
FLEWELL, A.	Postal Telegraphs, Bristol.
FOSTER, ARTHUR LE NEVE	East Hill, Wandsworth, S.W.
FRASER, CHARLES	Messrs. Warden and Co. Regent Street, Westminster..
FROST, ALFRED J.	5, Westminster Chambers, S.W.
GAMBLE, D. P.	26, Coleborne Road, South Kensington.
GAVEY, JOHN	Postal Telegraphs, Bristol.
GEORGE, EDGAR	Yokohama, Japan.
GIBSON, JAMES	Postal Telegraphs, Edinburgh.
GILPIN, GEORGE	Postal Telegraphs, Bradford, Yorkshire.
GOODMAN, H. M.	Endersby's Wharf, East Greenwich.
GOSSELIN, GEORGE	Indian Government Telegraphs, Colombo, Ceylon.
GOTT, JOHN	French Atlantic Telegraph Company, St. Pierre, Newfoundland.
GRANVILLE, WILLIAM P.	28, Hanover Square, Islington, N.
GRANT, JOHN	26, Old Broad Street, E.C.
GRAY, ROBERT	St. John's Park, Blackheath.
GROVES, WILLIAM	374, Euston Road, N.W.
GUENZEL, ERNST	25, Mount Street, New Charlton, S.E.
HALL, JOHN	Postal Telegraphs, Manchester.
HALL, WALTER	21, Aldermanbury, E.C.
HAMILTON, F. A.	Morden Wharf, East Greenwich.
HANCOCK, WALTER	10, Upper Chadwell Street, Myddleton Square.
HARBOROW, HENRY	4, Circus Street, Marylebone, W.
HARGREAVES, J. W.	Postal Telegraphs, Carlisle.
HASSARD, H. S., Lieutenant	66th Regiment, Sheerness.
HAYNES, J.	Postal Telegraphs, Gloucester.
HEAVISIDE, A. W.	Postal Telegraphs, Newcastle-on-Tyne.
HENLEY, GEORGE	47, Essex Street, Islington, N.
HEWETT, D. R., Commander R.N.	22, Arundel Street, Strand, W.C.
HIGGINS, FREDERICK	Exchange Telegraph Company, 17, Cornhill, E.C.
HIGGS, PAGET, L.L.D.	Penarth, Cardiff.
HILL, JOSEPH TRIPP	Postal Telegraphs, General Post Office.

HILL, THOMAS	Postal Telegraphs, General Post Office.
HILLIARD, AUGUSTUS	India Rubber Company, Silvertown.
HOELTZER, CHARLES	3, Great George Street, Westminster.
HORNER, ALBERT B.	General Post Office, St. Martin's-le-Grand.
HOOKEY, JAMES	Postal Telegraphs, General Post Office, E.C.
HUMBLE, GEORGE	Postal Telegraphs, Newcastle-on-Tyne.
HUMPHRIES, WILLIAM	Postal Telegraphs, Limerick.
HURLEY, EDWARD BUTLER	India Telegraph Department, Colombo, Ceylon.
IRMINGER, FREDERICK	Great Northern Telegraph Company, Hong-Kong, China.
INGRAM, J. W.	Postal Telegraphs, General Post Office.
ISHERWOOD, FRED.	Postal Telegraphs, 101, Cannon Street, E.C.
JAMES, SIDNEY	Postal Telegraphs, General Post Office, E.C.
JAMIESON, ANDREW	Charlton.
JENKIN, JOHN	Postal Telegraphs, Newark, Notts.
JOBSON, HOWARD C.	Dudley.
JOEL, H. F.	374, Euston Road, N.W.
JOHNSON, WILLIAM	Postal Telegraphs, 67, Portland Crescent, Leeds.
JONES, JOHN RYMER	Truro House, Westbourne Park, W.
JONES, T. J.	138, Regent Street, W.
JONES, J. C. D.	British Indian Extension Telegraph, Singapore.
KAARSBERG, SOFUS	7, Great Winchester Street Buildings, E.C.
KAISER, E. F.	6, Armstrong Terrace, New Charlton.
KEMPE, H. R.	Postal Telegraphs, Southampton.
KERRY, C. H.	Postal Telegraphs, Bristol.
KING, W. F.	Hooper's Telegraph Works, Mitcham.
KNIGHT, JOHN HENRY	Postal Telegraphs, Doncaster.
LAISTER, JAMES	101, Cannon Street, E.C.
LAVENDER, JOHN	Fairy Lane, Manchester.
LEE, ROBERT B.	Riband Post Manufactory, New Islington, Manchester.

LESSELS, DAVID	Postal Telegraphs, Edinburgh.
LLOYD, WILLIAM	Whitehall Club, Parliament Street, S.W.
LONDON, ROBERT	10, Vanburgh Road, East Greenwich, S.E.
LOWE, THOMAS	8, Great Winchester Street Buildings, E.C.
LUCAS, F. R.	3, Glen Mohr Terrace, Hyde Vale, Greenwich.
LUSTY, J. W.	Postal Telegraphs, Exeter.
MACKIE, ALEX.	15, Addison Road, Kensington, W.
MACKIE, S. J.	84, Kensington Palace Road, Bayswater, W.
MACLACHLAN, J. M.	Postal Telegraphs, General Post Office, E.C.
McGAURAN, D. E.	Victoria Telegraph Department, Melbourne.
McINTYRE, JOHN	Postal Telegraphs, Inverness.
McLEAN, JAMES	2, Ovington Square, S.W.
MANCE, HENRY	Indo-European Government Telegraph, Kurrachee.
MAY, JOSEPH	2, Trinity College, Church Street, East Greenwich.
MICHEL, FRANCISQUE R.	13, Rue de l'Ancienne Comédie, Paris.
MOLL, FREDERICK H. L. R.	Indo-European Telegraph Company, 16, Telegraph Street, E.C.
MOLLOY, B. C.	1, Elm Court, Temple, E.C.
MORGAN, CHARLES AMISS	Postal Telegraphs, General Post Office.
MORRIS, C.	(Scott and Co.) Hiogo, Japan.
MORRISON, G. JAMES	Engineer's Office, Clifton Extension Railway, Bristol.
MOSLEYN, P. J.	Postal Telegraphs, Newcastle-on-Tyne.
MUIRHEAD, ALEXANDER, D.Sc., F.C.S. (*Member of Council.*)	159, Camden Road Crescent, N.W.
MUNRO, JOHN	Hooper's Telegraph Works, Millwall, E.
MYGIND, JOHN	Newcastle-on-Tyne.
NAYLOR, J. E.	Postal Telegraphs, York.
NEALE, JOHN	North Staffordshire Railway, Stoke-on-Trent.
NEWSAM, THOMAS	Eastern Telegraph Company, Alexandria.

NIND, L. L.	Postal Telegraphs, Newcastle-on-Tyne.
NOBLE, MARTIN	Postal Telegraphs, Lancaster.
OAKSHOT, A.	Postal Telegraphs, Southampton.
OGG, F. WM.	166, Regent Street, W.
OLLARD, J. F.	Lloyd's Royal Exchange, E.C.
PAGE, ALF. S.	India Rubber Co., Silvertown.
PARKER, J. E.	St. Thomas, West Indies.
PARMITTER, ALBERT	Postal Telegraphs, Reading.
PARSONE, E. W.	175, Adelaide Road, N.W.
PARTRIDGE, G. NOBLE	Postal Telegraphs, Exeter.
PATTEN, FREDERICK A.	Indo-European Government Telegraphs, 55, Parliament Street, S.W.
PAYTER, J. W.	Telegraph Department, Melbourne, Victoria.
PENMAN, JOHN.	West India and Panama Telegraph Company, St. Thomas, West Indies.
PETERSEN, K.	German Union Telegraph Company, 4, Werder Strasse, Berlin.
PHILLIPS, W. R.	Paumbaum, India.
PINCHIN, WILLIAM HENRY	Postal Telegraphs, Exeter.
POMEROY, HENRY	Postal Telegraphs, Mullingar.
RAMSAY, JOHN, Lieut. R.E.	Postal Telegraphs, London Bridge, S.E.
REID, FRANK	Postal Telegraphs, Newcastle-on-Tyne.
RICH, H. R.	Indian Government Telegraphs, Calcutta.
RICHARDSON, H.	Postal Telegraphs, Douglas, Isle of Man.
RIDDLE, G. H.	Telegraph Account Branch, General Post Office, E.C.
RISCH, GUSTAV	9, Armstrong Terrace, New Charlton, S.E.
RITSO, F. C. G.	7, Lothbury, E.C.
ROBERTSON, J.	Postal Telegraphs, Bristol.
ROBINSON, F. L.	Direct Spanish Telegraph Company, the Lizard, Cornwall.
ROLLS, EDWARD T.	L. and S. W. Railway, Southampton.
ROWE, THOMAS	Postal Telegraphs, Manchester.
RUSSELL, FRANK	Woodfield Road, Harrow Road, N.

SALE, Lieut. R.E.	Chatham.
SAUNDERS, H.	Eastern Telegraph Company, 66, Old Broad Street.
SAX, J.	108, Great Russell Street, W.C.
SCHAEFER, LOUIS	Tokei, Japan.
SCHRAMM, OTTO	3, Great George Street, Westminster, S.W.
SHAW, JOHN	Postal Telegraphs, Bolton.
SHAW, Captain W.	73rd Regiment, 52, Hans Place, S.W.
SHEATH, ALFRED	Auckland, New Zealand.
SHEPHERD, F.	Postal Telegraphs, Brighton.
SIEMENS, ALEXANDER	Sample Steel Works, Hampton Street, Birmingham.
SIMPSON, GEORGE	Indian Government Telegraphs, Mannar, Ceylon.
SIVEWRIGHT, JAMES	Postal Telegraphs, Southampton.
SLATER, WILLIAM	Western and Brazilian Telegraph Company, Rio Janeiro.
SMITH, TOULMIN	3, Great George Street, Westminster, S.W.
SMITH, J. H.	2, Westminster Chambers, S.W.
SMYTHE, JOHN	Valentia, Ireland.
SPAGNOLETTI, HYLTON	4, Circus Street, Marylebone, W.
SPRATT, G. O.	Eastern Telegraph Company, Porthcurno, Penzance.
STEET, G. C., F.R.C.S.	Melbourne House, Rosslyn Hill, Hampstead.
STEVENSON, ED. ALF.	Telegraph Construction and Maintenance Company, Enderby's Wharf, East Greenwich, S.E.
STEWART, D.	Postal Telegraphs, Glasgow.
STOKES, JOHN SCOTT	Postal Telegraphs, Southampton.
STOUT, ROBERT	Postmaster, Lerwick, Orkneys.
STOW, G. E.	4, Adelaide Street, West Strand, W.C.
SYMINGTON, ROBERT STEVENSON	Glasgow.
STEVENSON, GEORGE	Eastern Telegraph Company, Alexandria, Egypt.

TANSLEY, WILLIAM	Postal Telegraph, Portarlington
TAPLIN, CHARLES	Postal Telegraphs, Cardiff.
TARBUTT, P. F.	The Avenue, Streatham.
TAYLOR, WILLIAM GRIGOR	Eastern Telegraph Company, Gibraltar.
THEILER, RICHARD	86, Canonbury Road, N.
THOMPSON, H. E.	Indian Government Telegraphs, Alipore.
TICEHURST, F. G.	Battle, Sussex.
TISLEY, S. C.	172, Brompton Road, S.W.

TOLMÉ, JULIAN H. M.I.C.E.	1, Victoria Street, Westminster, S.W.
TRENAN, EDWIN	Postal Telegraphs, Leeds.
TRUMAN, EDWIN THOMAS	23, Old Burlington Street, W.
TUBB, ALBERT	Postal Telegraphs, Southampton.
TUBB, W. H.	Indian Government Telegraphs, Indore, Central India.
TYLER, W. J.	106, Cannon Street.
UREN, JOHN GEORGE	Postal Telegraphs, Penzance.
VENNDT, C. F.	Great Northern Telegraph Company, Aberdeen, N.B.
VERNEY, Captain R.N.	Rhianva, Bangor, North Wales.
VYLE, SAMUEL	Postal Telegraphs, Glasgow.
WALTON, JOHN	Postal Telegraphs, Birmingham.
WATERS, R. J., B.A.	Telegraph Institution, Osnaburgh Street, N.W.
WATKINS, Lieut. R.E.	Gibraltar.
WATT, GEORGE W. M.	Government Telegraph Department, Mauritius.
WATSON, C., Lieut. R.E. (*Member of Council.*)	Chatham.
WEATHERALL, T. E.	Telegraph Construction and Maintenance Company, Greenwich.
WERDERMANN, E. H.	4, Prince's Street, Stamford Street, S.
WEST, GEORGE	Eastern Telegraph Company, Alexandria.
WIGAN, GORDON	Campden House, Kensington.
WILLMOT, JOSEPH	Postal Telegraphs, General Post Office, E.C.
WINTER, CHARLES	Postal Telegraphs, Southampton.
WOOLLEN, C. H.	Postal Telegraphs, Exeter.

Total Number of Associates 270

STUDENTS.

CHEESMAN, H. G. . . .	Postal Telegraphs, Hull.
GATEHOUSE, THOMAS . . .	374, Euston Road, N.W.
GRAHAM, WILLIAM JOHN . .	Henley's Works, North Woolwich, E.
HAYES, ALFRED	2, Westminster Chambers.
HOOPER, SAMUEL . . .	Tremerton House, Clapham Park.
MCKAIN, H. F.	
PALLISER, EDWARD . . .	

Total Number of Students . 7

TOTAL NUMBER OF MEMBERS.

Honorary Members	3
Foreign Members	57
Members	185
Associates	270
Students	7
Total . . .	522

THE

Society of Telegraph Engineers.

ESTABLISHED 1871.

COUNCIL.

PRESIDENT.

FRANK IVES SCUDAMORE, C.B.

PAST-PRESIDENT.

CHARLES WILLIAM SIEMENS, M. INST. C.E., F.R.S., D.C.L.

VICE-PRESIDENTS.

THE LORD LINDSAY.
LATIMER CLARK, M. INST. C.E.
R. S. CULLEY, M. INST. C.E.
SIR WILLIAM THOMSON, F.R.S., LL.D.

MEMBERS.

PROFESSOR ABEL, F.R.S.
W. H. BARLOW, M. Inst. C.E., F.R.S.
CAPTAIN COLOMB, R.N.
PROFESSOR G. C. FOSTER, F.R.S.
MAJOR MALCOLM, R.E.
W. H. PREECE, M. Inst. C.E.
ROBERT SABINE, C.E.
CARL SIEMENS, M. Inst. C.E.
LT.-COLONEL STOTHERD, R.E.
MAJOR WEBBER, R.E.
EDWARD WILDMAN WHITEHOUSE.
C. F VARLEY, M. Inst. C.E., F.R.S.

ASSOCIATES.

JOHN BORDEAUX.
PROFESSOR BOTTOMLEY.
H. G. ERICHSEN.

OFFICERS.

AUDITORS.

J. WAGSTAFF BLUNDELL,
FREDERICK C. DANVERS (India Office).

TREASURER.

MAJOR WEBBER, R.E., 101, Cannon Street, E.C.

HON. SECRETARY.

MAJOR FRANK BOLTON.

HON. SOLICITORS.

MESSRS. WILSON, BRISTOWS & CARPMAEL.

SECRETARY.

GEO. E. PREECE,
31, Bedford Street, Covent Garden, W.C.

OFFICE.

5, WESTMINSTER CHAMBERS, LONDON, S.W.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. II. 1873. No. 1.

The Tenth Ordinary General Meeting was held on Wednesday, January 8th, 1873, Mr. FRANK IVES SCUDAMORE, C.B., President, in the Chair.

THE PRESIDENT read his Inaugural Address as follows:—

GENTLEMEN,

While I thank you very cordially for the compliment which you paid me when you selected me for your President, I must at the same time express my regret that your choice has not fallen on some one better qualified to further the objects for which your Society was established. I had hoped that during this year, as during last year, this chair would have been filled by a man of scientific eminence, by a man capable of lending a real assistance at your discussions, capable of holding the balance between rival disputants, and capable of putting before you at the close of a discussion a *résumé* of the views expressed by those who had taken part in it. Such a President you have had, such a President you should always have, but unfortunately such a President you cannot have in me.

It has been my lot on more than one occasion to be placed, by the kindness and partiality of friends, in positions for which I have not the proper qualifications. For instance, I cannot row. I very much doubt if I can sit straight in a boat; and yet I am President of one of the best rowing clubs on the Thames. I cannot shoot—indeed, I cannot see above twenty yards in front of me; and yet I am the Honorary Colonel of a Rifle Regiment of no mean reputation. And now Fate, with equal irony, makes me President of your Society, though I know little or nothing of Telegraphy.

I believe, however, that I have been able to do something for the rowing club and the rifle corps, and if hearty good will to the members of this Society personally, and the keenest possible interest in its proceedings and its prosperity, will be of service, you and the Society will have the good will and the interest from me.

I am, indeed, bound to do all that I can to further the progress of your Society, because I feel that its growth ought to be and must be advantageous to the service to which I belong.

I am not now thinking merely of the benefits which the leading Engineers of the Postal Telegraph Department derive from their contact in this room with the Engineers of other services, although I believe those benefits to be great. I am looking beyond them to the great body of persons who are engaged in the practice of Telegraphy throughout the country, and who, though they cannot attend your meetings, may read the accounts of your proceedings—may learn therefrom that their occupation has attained to the dignity of a profession, that it has the constant attention of men of the highest scientific attainments, and that, great as the progress of Telegraphy has been during the last few years, the ultimate limits to its progress have not as yet been discerned by those best qualified to discern them.

The existence and growth of your Society cannot fail, as years go on, to attach the great body of telegraph employés throughout the country to their calling, to make them proud of it, and, by making them proud of it, to make them desirous for improvement, and to awaken in them a spirit of enquiry and a thirst for further knowledge, which must inevitably bring about improvements. Now, it is above all things necessary that this spirit of inquiry should be awakened in the great body of telegraph workers, and that they should be filled with a desire for further knowledge.

There are, I suppose, in this country from ten to fifteen thousand persons who are engaged daily in the practice of telegraphy. Most of them are young, and have before them all that period of life in which the mental powers are most active. In point of general intelligence and education, they are at least on a par with those members of their social grade who are engaged in other services. They have been taught to use, and they do use daily, instruments in the construction of which the greatest possible amount of mechanical ingenuity has been exerted, instruments which might well be expected to arouse

curiosity and stimulate thought, and to cause those in whose care they are, to ask themselves continually, "How is it that these machines produce the results which we see proceeding from them during every hour of our lives?" and never to rest until they found an answer to the question. And when they had got an answer to this question, we may be quite sure that other questions would follow; that the workers would want to know whether the same results could not be produced in other ways, or more simply, or more cheaply, and that from the once awakened curiosity, improvement and development would inevitably proceed.

Now I am afraid that in the minds of the great majority of the telegraph workers throughout the country, this laudable, this desirable curiosity, has not as yet been awakened. There are exceptions, highly honourable exceptions, some of which I shall hope to bring under your notice in the forthcoming year, but I am afraid that the great majority of telegraph workers regard their instruments merely as rather troublesome pieces of machinery, which are very easily deranged and very difficult of adjustment, and that their first, and, indeed, their only idea when one of these pieces of machinery gets out of order, is that somebody had better come from somewhere to put it right again.

Do not suppose that, in saying this, I am finding fault. I am merely describing what I believe to be a natural, not a blameable, state of things. There are in the world many millions of watches, but how few of the owners of those watches have ever even thought about the mechanism of the watch which they carry about with them all their lives through? *This* is a circumstance which concerns none but the owners of the watches; but the indifference—and I must beg you to understand that I do not use the word as a term of censure—the indifference of the telegraph workers to their instruments concerns the nation, and if your Society can even in part substitute interest for indifference, it will render a service to the nation. Probably some one will ask me whether the employers of telegraph workers might not give the requisite stimulus in the shape of promotion? To this obvious question, the answer is equally obvious. Promotion must be given to those who do their *allotted* work well and faithfully, and it is difficult to reward by promotion those who do more than they undertook to do, without taking something from those who have done all they undertook to do, but no more. Probably, too, some one will ask

why the employers of telegraph workers should not make some systematic effort to instruct the whole of the staff in all that it is desirable for the staff to know? I think the answer to this question is equally obvious. It would be useless to try and teach the whole of the staff at once, all that it is desirable for them to know. All have not, and cannot be expected to have, the capacity for learning all that it is desirable they should know, and of those who have the capacity, only those would learn readily, in whom there is at present an existing, though, perhaps, a dormant inclination to learn.

What we want to do is to arouse and quicken this dormant inclination, and I firmly believe that your Society will render us most powerful and valuable assistance. It is my hope and belief that, year by year, a continually increasing number of leading officers in the Postal Telegraph Department will enter the ranks of your Society; that, year by year, the attrition between their minds and the minds of other telegraph engineers who may be assembled in this room, will give rise to fresh developments of their science; and that, year by year, their example and their precepts will quicken, in an increased number of their subordinates, a desire, not only to do their work, but to understand, and improve, and simplify the means by which it is done. I have the best possible reasons for thinking that this will be so. I find my reasons in the circumstances which have occurred during the last three years. I find them in the efforts which the engineering officers of the Post Office have made during the last three years to keep pace with the requirements of the public. I say advisedly that they have made those efforts because they have found themselves to be the servants of the public, and that they have effected improvements in the system to which they would hardly have given attention if they had not become the servants of the public. They have for three years had to work under the sharp spur of necessity; they have had to provide for a constantly-increasing business; they have had to serve that most exacting of all masters—the public; and, day by day, as a greater and greater number of people have become interested and affected by the telegraph system, the public, our master, has become more and more exacting. Let me repeat what I have said elsewhere: "The Post Office is not popular because it is efficient; it is efficient because it is popular." It is efficient because it does a work in which all the members of a nation are interested, and which, if well done,

is very pleasing, but, if ill done, altogether intolerable to them. It is efficient because its master will have it so. With the cheapening of telegraphy has come the popularity of telegraphy, and with that has come a continually-increasing demand for greater facilities, greater accommodation, and greater efficiency. I look forward to the growth of this demand. I see that under its pressure the officers of the department will have, year by year, to make renewed efforts for the development of the system; that they must be continually seeking new appliances of their science; that in your Society they will find a most powerful co-operation and effective stimulus; and that by their aid and yours the great body of telegraph workers throughout the country will gradually be leavened with a knowledge from which further triumphs of science will inevitably result.

Mr. C. W. Siemens (Past-President) said he rose to propose a hearty vote of thanks to the President for his very able address. The President, in his opening observations, said, very modestly, he did not consider himself altogether qualified for his office, and he instanced the appointment which he held as President of a Rifle Brigade, and though he said he could not see twenty yards before him, he was, nevertheless, in spite of his modesty, obliged to admit that he had done good service to that brigade. Now, if that be so, how much more service would their President do to them, considering his far-sightedness in telegraph matters which he had shown above all other men in this country. He (Mr. Siemens) would say he looked upon Mr. Scudamore's presidentship as a period during which their Society would take a very great development. His great powers of organization, the well-known fairness of spirit in which he met all questions, and the frankness with which he was known to dispose of them, were guarantees to them that he would conduct this Society to greater proportions than it now enjoyed; and his official position, also, would help them in bringing to their Society, not only the members belonging to the great Postal Telegraph system of this country, but also those of other countries. Their brethren in other countries would see that this Society had taken solid root, that it is *the* Society of Telegraph Engineers, and not belonging to this body would be tantamount to being an outsider. He congratulated them most heartily upon the acquisition of Mr. Scudamore as their President, and in moving a vote of thanks to him, he begged to move, also, that he would allow his address to be printed in the Journal of their proceedings.

Mr. E. Graves said that in seconding the motion of Mr. Siemens he would remind the members that they had a divided duty to perform. They had, in the first place, to welcome, and he was sure they did heartily and cordially welcome, their new President in the assumption of his office, and that they were all convinced he would most thoroughly discharge the duties of it; and they had also to recognise the services, the vast benefits, he might say, which the Association had derived from the action of its late President. There were some things in connection with this Society which, from their very success, showed that the labours of those who conduced to that success were worthy of the heartiest recognition. The establishment of a society such as this, in its first stages, was encumbered with many difficulties, which he need not stop to explain; and the fact of such difficulties having been overcome was proof of the exercise of no common capacity, and no ordinary labour, on the part of those under whose auspices that success had been achieved. But it was not merely in the initiation of the Society, not merely in bringing it into existence, but, in many ways, by forwarding, and developing and increasing its prosperity, that they were indebted to the assistance afforded by the first President of the Society. It must be remembered that one great difficulty necessarily contingent upon the establishment of an association having for its object the extension of telegraphic knowledge, arose from the widely diffused localities in which the persons interested in the science are resident. Telegraphs, so to speak, cover the world, and over the whole surface of the globe we find scattered those whose interests and inclinations are connected with the spread of telegraphic knowledge. If, therefore, this Society did not embrace a wide circle, it was evident that the public service it would render must necessarily be limited. The subject itself is so vast, ramifying into so many branches, that no single individual, and no small number of individuals, however great their knowledge or interest, can have devoted the attention to various minutiæ of this interesting science which is necessary to a complete store of general knowledge. The objects of the Society are so wide that the circle embraced must be a large one. Moreover, there was one important thing to be considered before permanence could be secured. Financial resources must exist, and financial resources could only be obtained by interesting a considerable number of persons in the Association. Within a year they had started from nothing to a body numerous enough to lead to the belief that in

progress of time the Society would embrace nearly everyone who had anything like a living interest in telegraphic proceedings. Then, too, a young society was something like a juvenile couple contemplating matrimony and waiting on providence for the means. The first great difficulty to be encountered was the provision of a local habitation, a difficulty which was sometimes met by resorting to the convenient but not very dignified process of taking lodgings. This Society, however, had been secured from that drawback mainly by the action of their first President. They had not only a name but a local habitation of which they might well be proud, and for which they had to thank the Institute of Civil Engineers—the parent so to speak of all practical scientific societies; and for the action which induced them to give them this residence they had mainly to thank their first President. He need not remind them of the successful *soirée* held in the rooms of Lord Lindsay, and the complete display of Telegraph apparatus which was there made, nor, further, of the magnificent exhibition which took place subsequently at the Albert Hall, an exhibition of a character that could not fail to call forth general admiration. Lastly, there was one thing of great importance which he could not pass over without comment. He had said it was necessary to the success of a society like this that it should embrace a wide circle. The difficulty of effecting that result was the extent to which the persons interested were scattered over the world. Everything depended upon the clear and full publication of the proceedings in this room. Unless members who had little opportunity of attending the meetings were in possession of full records of what happened it was evident that their interest in the Association must soon flag; and, moreover, the large outside constituency who could not visit London would not be interested in sending the views and observations that might occur to them, unless they knew that in some shape they would be promulgated. Hence, at the outset, it was evident that something was wanted beyond what the subscriptions of members supplied, to provide for the issue of transactions in a satisfactory and complete manner. A Publication Fund was organised; to that fund Mr. C. W. Siemens subscribed the sum of £100 at the commencement of its operations, and Mr. Scudamore had likewise given liberal assistance to the project of diffusing the records of their proceedings, by an annual subscription of 10 guineas for that purpose. The moral of this was "go ye and do likewise." He was sure what he had

mentioned, although it was known to them before, was sufficient to justify him in asking them for a recognition of the great services of their first President. With reference to Mr. Scudamore, who assumed the chair for the first time, he would say that the address he had delivered, the new light he had thrown upon the operations of this Society, and the new principles he had introduced into the telegraphic business of the country, were contradictions of the modest terms in which he himself deprecated his own fitness for the position. He had described the reflective effect of their proceedings, and had shown how greatly they might stimulate the public service by inquiry into, and the development of, those particular branches of science, which some of them had perhaps regarded rather as an intellectual exercise than as having a bearing upon the improvement of the working powers of a great department of the public service.

Mr. C. W. SIEMENS said, as the mover of the vote of thanks to the President for his address, he had to come before them also as a giver of thanks, and especially to Mr. Graves, for the very kind manner in which he had spoken of his (Mr. Siemens') past services, and to the meeting at large for the kind manner in which those remarks had been received; and the more so for the very cordial support which he had received during the term of his office from them all, in aiding him in his endeavours to promote the interests of the Society to the best of his ability, but which efforts would have been unavailing without their effectual support. He thought he need not revert to the proceedings of the past year. They had all done their duty—that he would say of all the Council and Officers around him. That effectual support had made it possible to advance the interests of the Society, and had tended to bring it to the point at which it now stood. He thanked them all for their kindness, and he would now put it to the meeting, that the President be requested to allow his address to be printed.

THE motion was unanimously adopted.

Mr. VON CHAUVIN then read a communication from Dr. WERNER SIEMENS, "On the Measurement of Resistances." *

THE SECRETARY next read a description of Tray Battery for Syphon Recorder, communicated by SIR WILLIAM THOMSON. †

THE PRESIDENT said there were two relatives of Sir W. Thomson

* Printed in Journal No. 3, page 407.
† Printed in Journal No. 3, page 403.

present, viz: Mr. Bottomley and Mr. King, who would be happy to reply to any inquiries on this subject.

Mr. C. W. Siemens begged to ask what was the resistance of this battery? As he understood, it was filled for the immediate use to which it was to be put, and he believed its use was for lines of very high resistance. It would be interesting to know what was the resistance of a battery of this construction containing so little sulphate of copper, and what was its electro-motive force; also, whether these batteries had been used for any length of time and with what practical result.

Mr. Bottomley replied that the resistance of one of these cells, the plates being about 21 inches square, was 1·9 ohms. The battery had been used for five or six months, and during that time the resistance had been about 1·9 ohms.

Mr. Culley asked if it had been constantly in use?

Mr. Bottomley replied: Yes, working day and night.

Mr. Culley inquired how much sulphate of copper was used per week?

Mr. Bottomley replied: It depended upon the work it was put to.

Mr. C. F. Varley believed he could answer some of the questions put. He had worked a good deal with these batteries during their construction. The resistance of these large cells he said was somewhere about 1·7 ohm. It varied with the strength of the sulphate. The cells were something near two feet square. The object of the battery was to work the recorder magnet, in which the principal electromagnet was required to be kept charged for days together. The recorder consists of a large electromagnet of great power, with a moving coil between the poles, upon the plan patented by himself, several years ago, and, therefore, great magnetic intensity was required; and these large batteries were constructed to dispense with the trouble of the Groves' battery, whose life is of short duration, so as to get a battery to work for weeks with a large current and small variation of power. The quantity of sulphate of copper does not interfere with the intensity of the current. This battery was, in fact, an extension of the old gravity battery, which he (Mr. Varley) introduced and patented in 1855. One of the things which Sir W. Thomson discovered in this battery was, the moment it was set to work, from the negative element, from some unexplained reason, gave off hydrogen gas, and this,

flowing under the zinc plate, put a large surface of zinc out of work, and thereby caused irregularity in the resistance of the battery. In order to obviate that, Sir W. Thomson cut the zinc into a grating, through which that gas escaped. It was, in fact, an enlargement of the gravity battery. This plate was cut into a great many bars. They could not use one large piece of zinc without having great resistance, by reason of the distance between any conical zinc and the copper. Sir W. Thomson had introduced a number of bevelled bars, so that the distance between the copper and the zinc was very small. There were several difficulties to be overcome in this battery; one was, that when fitted with copper trays, they were eaten through by the sulphate of copper, at the junction between the two solutions. The question was, how to obviate that difficulty? He believed he was the first to suggest the use of lead for the negative metal. That, likewise, gave rise to a difficulty, inasmuch as the moment the copper sulphate was poured on to the lead, chemical action took place, the lead having great affinity for sulphuric acid, and then a film of oxide of lead was thrown down; therefore, it was necessary to make complete metallic connection with the lead, and for that purpose a little piece of copper was soldered on the lead, and then there was spread over the bottom of the tray, Dutch metal, to form a film of copper in metallic contact with the lead, then the copper and zinc solutions did not eat their way through. This battery had been in use at Porthcurno, and was now used at Lisbon, and on various lines down the Mediterranean, on the Red Sea, and across to India. It had worked for weeks together without requiring cleaning or attention, beyond putting occasionally a little sulphate of copper into the cells.

The President then called upon the Secretary to take up the next business for the consideration of the meeting, viz., a communication "On a new form of Joule's Galvanometer,"* also from Sir W. Thomson.

The communication having been read,

Captain Malcolm asked what was the peculiarity as to the suspension of the needle of which Sir W. Thomson spoke.

Mr. Bottomley replied that there is a ring of aluminum with a bar across. From the centre of the bar a very fine fibre is suspended to the top of the case. These coils slide along a brass bar, with a

* Printed in Journal No. 3, page 392.

triangular groove in it; and by altering the distance of the coils from the needle, the deflection of a large battery is very readily read off on the scale.

Mr. Siemens remarked—Simply to diminish the effect. You do not measure each by the distance.

Mr. Bottomley: No; we measure by altering the distance of these coils, and the brass bars are connected by a sliding contact.

Mr. Siemens: You can regulate the sensitiveness of the instrument *ad libitum* by the assistance of these coils?

Mr. Bottomley: Yes; as the divisions are in proportion to the tangent, the resistance of the battery is got by the ordinary electrometer.

Mr. Culley remarked that the division in the scale of tangents had been adopted in the Post Office for many years, and he was glad to find it had the sanction of such an authority as Sir W. Thomson. Some persons were inclined to question the accuracy of it, but he never saw any weight in the objections. The mirror was also employed for reading off, to neutralise the effect of the parallax. It had been found very serviceable indeed.

The President said there were one or two other communications from Sir Wm. Thomson, but time would not permit of justice being done to them to-night; and he suggested they should be reserved for another evening. There was, however, a short communication from Mr. Graves, of Valentia, which the Secretary would read, and that would agreeably fill up the time at their disposal.

The Secretary read some notes "On Lightning and Lightning Conductors," * by Mr. James Graves.

Major Stotherd understood Mr. Graves to state that the greater the number of lightning conductors the less was the number of storms. He would state that in Canada, where he lived for some time, nearly every house had a conductor, yet in no place had he ever seen more thunder storms.

The Secretary said the statement in the paper was—the less would the presence of lightning be seen—not that there would be fewer storms.

Mr. Ayrton said the best proof that it was considered that lightning does not go up was that lightning dischargers are made with points, the object being to discharge the electricity, as Mr. W. H.

* Printed in Journal No. 3, page 413.

Preece showed in the experiment of a pin with the streamers of paper electrified. The writer attached more importance to the new theory than he (Mr. Ayrton) thought was due to it, because the dischargers have points for the purpose of dispelling the clouds by allowing the electricity of the opposite cloud to come up from the ground.

Mr. C. F. VARLEY said in the year 1868 he made a number of experiments in Switzerland with the view of ascertaining whether it was true or not that clouds were sometimes charged with negative electricity, and for that purpose he took an electrometer and spent some weeks on the Rigi. On two occasions, immediately after heavy falls of snow, and when the snow was actually falling, he found negative electricity at the mountain top, while in his garden near London he had frequently obtained negative electricity from the lower strata of the air, being partially discharged in passing over the houses, while really the atmosphere above was positive. Working on the top of the mountain he got nearly free from all surrounding influences; and on one occasion he found by the electrometer a charge equal to 1800 cells of a Daniel's Battery, and on another occasion equal to 1400 cells, and on another something much more than his electrometer could measure, but it did not last long at that potential. His impression from what he observed was that the negative clouds had previously been made so by a highly charged positive cloud at a great elevation passing over a cloud lower down which was dropping snow; each particle of which fell charged from that cloud, in consequence of the inductive action of the upper cloud, which was more charged with positive electricity. The consequence was, when this upper cloud moved away it left the lower one negative to the earth. He could not conceive that nature should give a negative charge to a cloud, except by reaction, because there was very little doubt in his own mind that the electricity of the air was due to the rotation of the earth, which was itself a great magnet, rotating in a highly attenuated atmosphere, which was, comparatively speaking, a conductor of electricity. If (said Mr. Varley) you rotate a magnet, so that the poles of it rotate in the direction in which the earth is moving, in a conductor, you will find that the two poles of that magnet will be positive, and I have very little doubt, that the way in which the atmosphere becomes charged is this—the rotation of this magnet in space produces positive electricity at the two poles, and this is com-

municated to the atmosphere, in the form, possibly, of Aurora Borealis, and as the lower stratum of the atmosphere is, by comparison, an insulator, we may expect all the upper regions, except under exceptional circumstances, to be positive to the earth.

With regard to providing lightning conductors, he was not present at the concluding discussion on Mr. Preece's paper. Much was yet to be said on the subject of lightning conductors, and he hoped it would be still more fully discussed. Lightning conductors were of two kinds —one was to preserve a building from the discharge when struck: but the other, the original invention, was to discharge the cloud silently, and not allow the flash to take place. It was very essential in the case of powder magazines to avoid the flash of lightning striking the conductor near the powder magazine, because of lateral discharges. The discharge was very instantaneous. Wheatstone had shown it was less than the hundred thousandth part of a second. It was so sudden that they could scarcely get a conductor so well united to the earth, that it would not leave the conductor immensely charged at the moment of being struck. The best method of meeting that difficulty, he was aware of, was that which he adopted at the Valentia Station, viz: to put up two conductors on each side of the house, run a wire across, and connect the base of the conductors to a circle composed of stout wires, the circle surrounding the house. The moment a flash struck this conductor the whole circuit was of nearly the same potential, and reduced to a minimum the tendency to lateral discharge. But when they came to the original idea of Franklin, viz: that of discharging the cloud silently, it would be found that a point was an insufficient instrument for that purpose. One of the experiments he tried on the Rigi was to prove the difference of effect between flame and the metallic point as collectors of electricity. He found that when the air was charged more than 3,600 Daniel cells, as indicated by the flame, and which was the limit of the electrometer, he could only get a power of something like 40 cells with a point, and in experiments at home he found that a point was powerless to discharge electricity until the tension got up to several thousand cells; but flame would draw off the electricity from a potential of less than one cell. The question was put to him by Captain Galton a short time ago, what he would suggest for the protection of powder magazines, when he expressed his opinion that the safest means of preventing lightning from striking powder magazines, and the best means of discharging

a cloud would be to have at no great distance from the magazine two or more pipes connected, with gas works, always burning a large flame, and he did not believe a flash of lightning would ever strike near the magazine, because the cloud would be silently discharged. This point could be determined by a series of experiments not of a costly nature.

The meeting then adjourned, and a ballot for new members took place.

The following Candidates were balloted for and declared duly elected:—

As Foreign Members:—

E. A. Calahan, 17, Cornhill.
Cyrus Field, New York.

As Members:—

Thomas Fuller, 119, Gloucester Terrace, Hyde Park.
Frank Lambert, 15, Great Castle Street, Regent Circus.

As Associates:—

Thomas Bewick, M.I.C.E., 4, Queen's Square, Westminster.
W. M. Bullivant, 114, Fenchurch Street.
W. H. Davies, 17, Cornhill.
John Fahie, Indo-European Government Telegraph, Jask, Persian Gulf.
John Hall, Post Office Telegraphs, Manchester.
Commander D. R. Hewett, R.N., 22, Arundel Street, Strand.
S. L. Nind, Post Office Telegraphs, Newcastle-on-Tyne.
J. W. Payter, Telegraph Department, Melbourne.
G. C. Steet, F.R.C.S., Melbourne House, Rosslyn Hill, Hampstead.
Commander Verney, R.N., 22, Arundel Street, Strand.
T. E. Wetherall, Telegraph Construction and Maintenance Company, Greenwich.

The Meeting then adjourned.

The Eleventh Ordinary General Meeting was held on Wednesday, January 22nd, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

THE CHAIRMAN, in opening the proceedings, said he had a matter of some interest to mention to the meeting. A few gentlemen had associated themselves together, in their private capacity, and quite apart from their membership in this Society, with a view to endeavour to obtain by gift or purchase the portraits of some of the most eminent men connected with telegraphy. They must all feel that it was a great privilege to live in the earlier history of their art, and to know personally the fathers of telegraphy; the men whose names would be in future ages "familiar as household words."

But it was all the more their duty to think of those who would come after them, to whom it would be of great interest and value to possess the portraits of such eminent men; endeavours had therefore been made to collect funds, and by purchase or other means to obtain those portraits. At some future time the members might be asked to assist in this good work; but in the mean time, the individuals to whom he referred would be happy to receive the photographs or autograph letters of eminent electricians which would be carefully preserved; it was the intention to procure the portraits of such men as Sir F. Ronalds, Sir Chas. Wheatstone, Sir William Cooke, Professor Morse, Sir William Thomson, and others, whose names would readily occur to them. The one about which they naturally felt most solicitous was that of Sir Francis Ronalds, who being more than 90 years of age they could not hope to have much longer amongst them, and he was happy to say they already had the promise of the portrait of that distinguished pioneer of the science from his relative Mr. Carter.

THE SECRETARY then read a communication from Sir William Thomson, "On the Measurement of Electrostatic Capacity." *

* Printed in Journal No. 3, page 394.

The following paper was then read, "On a Common Source of Error in the Measurement of Currents of Short Duration when Using Galvanometers with Shunts," by Mr. Latimer Clark.

It has long been noticed that considerable discrepancies appear to exist in the relative capacities of different condensers, when measured by different individuals, and much inconvenience has resulted therefrom. These discrepancies have been usually attributed to some peculiarity in the form or rate of vibration of the needle of the galvanometer, with which the measurements were made, or to some supposed actual change in the capacity of the condensers themselves.

A similar discrepancy has been noticed in obtaining the ratio of batteries for testing the core of submarine cables; in this operation it becomes necessary to ascertain the ratio between the electro-motive force of the powerful batteries with which the core is tested, and that of the single cell or cells by which the "constant" of the instrument is obtained, and this is usually done by comparing the deflection obtained from the discharge of a condenser with the single cell, with that obtained from the same condenser with the whole battery, suitable shunts being introduced so as to bring the swing of the needle in both cases within convenient limits for reading.

In order to avoid the inconvenience and loss of time of this method of obtaining the constant of the instrument, I have long been in the habit of employing a very high resistance in circuit with the battery, in fact, a resistance of a million ohms. This resistance is so great that by means of shunts, the deflection from a very powerful battery may be so reduced as to be capable of being read off directly on a highly sensitive galvanometer, and the same battery being afterwards employed in testing the cable, the question of its electro-motive force does not enter into calculation.

In the same way the ratio of the electro-motive force of a large battery and of a single cell is, of course, readily obtained.

Now it was found that whenever this comparison of the force of two batteries was made by the system in question, and also by means of the swing of a needle produced by the discharge of a condenser in the manner first indicated, there was always a considerable

discrepancy in the values thus ascertained, it was found that the constant obtained by the condenser method was always smaller than that obtained by the resistance coil method, the difference being commonly two, three or four per cent. On endeavouring to ascertain the cause of this discrepancy, it was further discovered (by using known values) that the results given by the resistance coil were correct, and those obtained by the use of condensers were wrong.

This result was so uniform that much interest was felt in determining its cause, and after some preliminary trials, a careful investigation of the subject was made at my request by Mr. Herbert Taylor and Mr. Frank Lambert. After considerable research the error was finally traced to the use of shunts, as may be shown by the following simple experiment.

The discharges from two independent condensers of nearly or exactly equal capacity, charged by the same battery, are observed on the same galvanometer, and deflections, which we will call d and d', are obtained. The galvanometer is then shunted by a resistance coil, having exactly the same resistance as itself, and the joint discharge from the two condensers combined, when charged to the same potential as before, is observed.

If the ordinarily accepted expression for the multiplying power of a shunt $\left(\frac{g+s}{s}\right)$ were correct for a sudden throw of the needle as it is for a permanent current, the deflection so obtained ought evidently to be the same as that obtained in the first instance, that is to say, the mean of d and d', but, instead of this being the case, a considerably smaller value is always obtained from the joint condensers when the shunt is employed, showing that less than the calculated proportion of the current passes through the galvanometer. That this is the case can be prettily shown by the following experiment:—

Two galvanometers are taken of the same resistance, and, approximately, of the same sensitiveness (these adjustments being easily made by means of a rheostat and directing magnet), and the discharges from the separate condensers are observed on each galvanometer. The two galvanometers are then connected in parallel circuits, so that the one galvanometer forms a shunt, or derived circuit, to the other.

Now, in this condition of things, if the joint discharge from the two condensers be observed on one of the galvanometers (A), there is no longer any error, but correct results are obtained.

If, however, the needle of the other galvanometer (B) be held rigid the reading on (A) becomes at once too small, precisely as in the case when an ordinary shunt is employed.

The cause of the error now at once becomes obvious, and is in full accordance with known laws. The movement of a needle tends to induce in the coil within which it is suspended, a current in the opposite direction to that producing its deflection. A larger proportion of the whole current, therefore, passes by the shunt which is not subject to this influence. When the two galvanometers were used, the movements of the needles within their coils being similar, each of them counteracted the other's influence.

This error only becomes visible on galvanometers of considerable sensitiveness, and of high resistance. For example, with a galvanometer of 7,000 ohms, and with a shunt of equal resistance, an error of over 2 per cent. is frequently produced, but, where the disproportion of resistances is considerable, a much larger error is produced, the amount varying with the sensitiveness of the galvanometer.

This source of error is not at all generally known, or, at any rate, is generally disregarded, and it is probable that many errors have been thus introduced in measuring both the resistance and the capacity of cables; the discrepancies which are known to occur in the measurement of condensers by different individuals, may be probably referred, in many instances, to the same cause.

The error of course does not exist when the measurement is made by a differential galvanometer, one of the coils being shunted until the needle remains unaffected by the simultaneous discharge of the two condensers under comparison—for there being no movement of the needle there is no electromagnetic influence. De Sauty's method of comparing condensers by an arrangement similar to a Wheatstone's Bridge is also free from this source of error, but there are difficulties in measuring accurately by either of these systems well known to practical electricians which are not easily overcome.

The correction of this error becomes obviously a matter of much interest to electricians; on some instruments the error is, to a great extent, compensated by employing the formula generally used in allowing for the diminution of the amplitude of the needles deflection due to the resisting influence of the air, which is in its roughest form

$$D = d + \tfrac{1}{4} (d - d')$$

where d is the first swing of the needle, and d' the return swing in the same direction.

Mr. Hockin, who has investigated the problem, finds, however, that the law in which the error varies with the power of the shunt in case of any particular galvanometer is as follows:—

$$\text{True deflection} = \text{observed deflection } (1 + \frac{C}{g + s})$$

where C is a constant derived from experiment in the manner hereafter described, and which varies with the state of sensibility of the instrument at the time of experiment.

The co-efficient C may be obtained by taking a deflection from the discharge of a condenser with a single cell—another deflection d' from the discharge from a nearly equal cell—then combine the cells in series, and take a discharge D from the two together, using a shunt equal in resistance to the galvanometer. We then have

$$\frac{d + d'}{2} = D\ (1 + \frac{C}{g + s}) \text{ or } = D\ (1 + \frac{C}{2\,g.})$$

$$\text{whence } C = \frac{g}{D}\ (d + d' - 2\ D.)$$

If the deflection from a single cell is not large enough for convenient reading, of course two or more cells may be used to obtain d and an equal number to obtain d'.

There is another and perhaps preferable method of obtaining these values—this is by allowing the current from about four or six cells to flow constantly through a high resistance coil, say not less than 10,000 ohms—whatever the potential be at the end nearest the battery it will of course be exactly half the potential at half the resistance, say at 5,000 ohms. A condenser being charged first at the point of full resistance, and then at the point of half resistance, will give the values of D and those of d and d' (which are in this case identical) with great accuracy; from which values C may be calculated by the formula as above, or by $C = \frac{2\,g}{D}\ (d - D)$

The general lesson to be derived from these considerations is however, that it is better in testing submarine cables to avoid the use of condensers for comparing batteries with their standard cells, and to make use of very high resistance coils, and the author has for some time past recommended the use of these high resistances (commonly 250,000 ohms) in all cases in which he has been consulted.

As a numerical example of finding the value of C, a galvanometer

of 7,000 units, without shunt, gave a discharge from a certain condenser charged with two cells of 422 divisions, the same when charged with four cells, or, to double the potential, with a shunt equal to itself gave 413 divisions, hence $C = \frac{14,000}{413}(422-413) = 305$.

Applying this value to a given case of finding the correct ratio between a large battery and a single cell, we have galvanometer 7,000 ohms, with shunt 7,000 ohms (multiplying power = 2) discharge from one cell 450 divisions. The discharge from the same condenser with 200 cells and a shunt of 17 ohms (multiplying power = 412.8) gave 445 divisions.

By the ordinary calculation, the battery ratio would be expressed by

$$\frac{445 \times 412 \cdot 8}{450 \times 2} = \frac{18,368}{900} = 204 \cdot 1$$

The corrected value for these two discharges would be:—

For the shunt of 17 ohms—

$$1 + \frac{C}{7,017} = 1 + \frac{305}{7,017} = 1 \cdot 0435$$

For the shunt of 7,000 ohms—

$$1 + \frac{C}{14,000} = 1 + \frac{305}{14,000} = 1 \cdot 0218$$

The corrected ratio of the electromotive force of the battery to the single cell is therefore—

$$204 \cdot 1 \times \frac{1 \cdot 0435}{1 \cdot 0218} = 204 \cdot 1 \times 1 \cdot 021 = 208 \cdot 4$$

The general expression for this correction is—

$$\text{Ratio} = D\, s' \frac{(g + s + C)}{d\, s\, (g + s' + C')}$$

Where D is the deflection with the whole battery—

s. The shunt used.
d. The deflection with the smaller battery.
s'. The shunt used.
g. The resistance of the galvanometer.
C. The correction first described.

The following paper was also read, "On an Instrument for Measuring Differences of Electric Potential," by Mr. Latimer Clark.

The Instrument which I venture to bring before the notice of the Society this evening is one which I have had in use a great many

years, and the principle of which was in fact described in the Government Report on Submarine Cables in 1861. It has also been described in subsequent works, but it has not, I believe, become at all generally known or used. I have, however, employed this instrument very largely, and have found it so useful that I can only ascribe the neglect which it has received to a want of knowledge and appreciation of its merits.

I have been in the habit of calling it an "Electric Potentiometer" a name which is of course equally applicable to any instrument for measuring differences of electric potential.

It is well known that if a current of electricity be allowed to pass through a wire of uniform conductivity, and if the wire be divided into equal lengths, as, for example, *a*, *b*, *c*, *d*, the potential at each of these points will increase in proportion to its distance from *d*.

The instrument in question depends on this principle, and is thus constructed (see diagram next page).

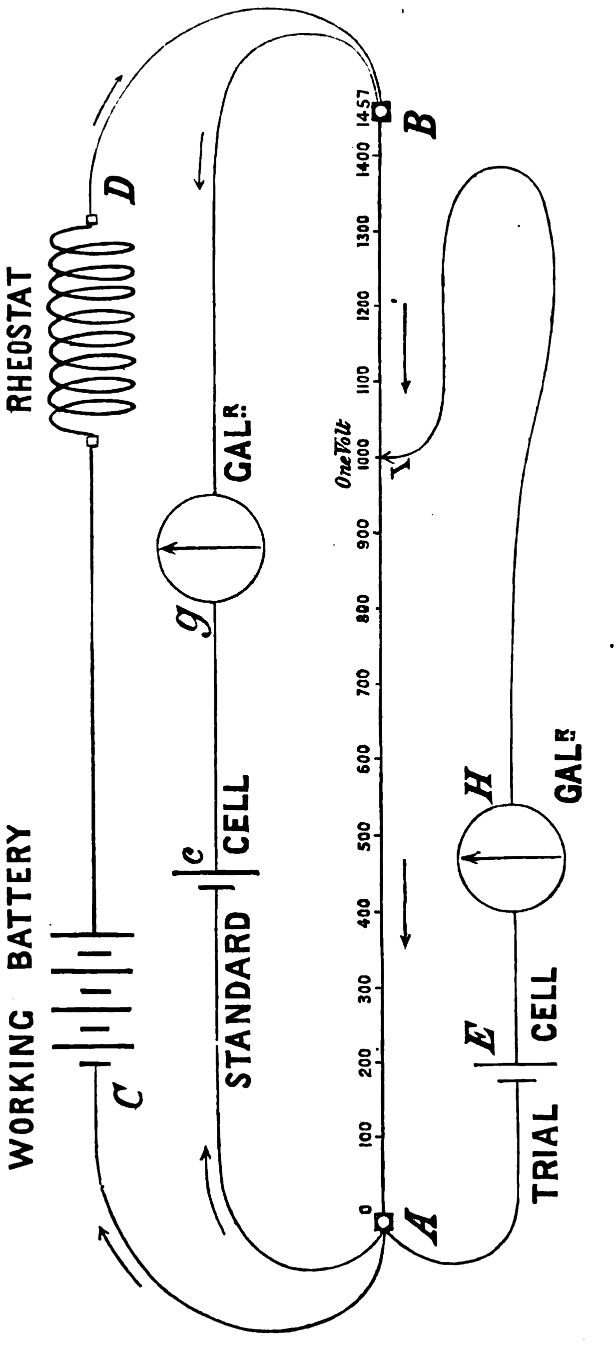
A platinum iridium wire of uniform calibre is stretched between the terminals A B, which are fixed on the board, the space between them being divided into 1,000 equal parts, or rather 1,000 parts of equal resistance.

A current of electricity is kept constantly flowing through this wire from a battery C, which is connected with the terminals A B. The strength of this current can be varied at pleasure by means of the rheostat or resistance coil D, which is interposed in circuit with it.

A smaller battery C, or a single cell, is also connected to the same terminals, in such manner that both batteries tend to send a current through the wire A B in the same direction.

Now if the difference of potential, maintained by the large battery between the terminals A B be greater than that of the single cell, the large battery will of course overpower the single cell and send a reversed current through it.

If on the other hand the difference of potential be less, the large battery and the single cell will both of them send a current through the wire A B. In practice, however, the strength of the battery C, and the resistance D are so adjusted that difference of potential at A and B are exactly the same as between the poles of the standard cell, in other words is equal to its electromotive force, so that no current passes through or from the cell C. This is shown by a Thomson's galvanometer *g* which is placed in the circuit, the needle of which is maintained at zero by adjustment of the rheostat.


WORKING BATTERY
RHEOSTAT
C
D
STANDARD CELL
c
g
GAL^R
TRIAL CELL
E
H
GAL^R
One Volt
A
B
0
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1457

We have then therefore the terminals of the wire A B kept at a difference of potential, exactly equal to the electromotive force of the standard cell *c*, and this without that cell doing any work, and quite independently of its internal resistance. The wire being divided into 1,000 equal parts from A to B, we have furthermore that electromotive force divided into 1,000 equal parts. The potential of the wire at A may, if required, be made *o*, by connecting it with the earth, but this is not generally necessary.

Now either calling or actually making the potential at A $= o$, if we wish to determine the force of any other element E, we connect its similar pole to the terminal A, and the other pole through a second reflecting galvanometer H to a sliding piece I. By means of this sliding piece contact can be made at any point along the wire A B, and the point on the wire is readily found at which the potential is the same as that of the trial cell, and consequently no current passes through the galvanometer H. In this case the reading gives the value of the trial cell in terms of the standard element.

It is of course necessary that the standard element should have a *higher* force than that of the trial cell, and this may be effected by using two cells for the standard, in which case it is convenient to divide the wire into 2,000 equal parts; the author has in some cases used as many as 250 cells for a standard of comparison.

I have thus far described the wire A, B as being stretched in a straight line, but as it would be inconvenient to use a wire of great length in this form, I have usually wound the wire spirally on an ebonite cylinder about six inches diameter; the edge of the cylinder is divided into 1,000 equal parts, and there are twenty turns on the cylinder, so that the whole wire is divided into 20,000 equal parts, the terminals A B are replaced by massive ends and axles revolving in cups of mercury, and the sliding contact piece I is traversed by a screw and handle which also revolves the cylinder, the final contact being made by the pressure of the finger. I may here observe that the 20,000 parts into which the cell is thus divided, is by no means the limit of the subdivision—if the galvanometer H be a very sensitive one, a movement of one division along the wire will cause a deflection of 50 or more divisions on the galvanometer, and in this way, we may, if required, subdivide the tension of a Daniell's cell without difficulty into a million parts.

Although I have used mercury contacts at the axles, there is no

reason why double platinum springs should not be employed for these contacts. One thing being essential, viz, that the contact springs of the main battery should be quite independent of those of the standard cell, whatever variations of resistance may be thus introduced have of course to be compensated by the rheostat; but these variations when thus compensated do not cause any error, and they are so slight as to occasion little trouble in actual practice.

I need scarcely point out the similarity between this mode of measurement and the well-known arrangement of M. Poggendorf. M. Poggendorf opposes two batteries by connecting their similar poles to each other, interposing between the two positive poles and the two negative a variable resistance, and he adjusts this resistance until the stronger of the two batteries has its potential at the point of junction reduced to exact equality with that of the weaker battery, through which no current passes. Knowing the resistance interposed, and that of the stronger battery, we can thus determine its value in terms of the other.

The improvements which I have effected on this system are—

(1) The arrangement of the third battery, by which both the trial element and the standard with which it is compared are kept inactive, so that their resistances need not be known.

(2) The provision of a standard element of determinate value (described below); and

(3) The introduction of a graduated scale, the degrees on which are numbered in absolute electro-magnetic units.

The standard battery, which first suggests itself, is naturally the Daniell's cell, but this is very variable and quite unfit to be used as a standard. In order to render this instrument perfect and to be able to assign a definite value to its readings, it is obviously necessary to know the exact electromotive force of the standard cell employed. It is well known that all the batteries at present in use vary considerably in their electromotive force however carefully they may be prepared. It became, therefore, extremely desirable to discover some new form of battery which should give a uniform constant of electromotive force, and that that force should be determined in absolute measure. The author has devoted his leisure for many years to the solution of this problem, and last year read a paper before the

Royal Society,* in which he described a form of battery which possesses the required constancy and uniformity to the fullest extent.

This battery is composed of pure mercury as the negative element, the mercury being covered by a paste made by boiling mercurous sulphate in a thoroughly saturated solution of zinc sulphate, the positive element is a small cylinder of pure zinc resting on the paste. The best method of preparing it is to dissolve pure zinc sulphate to saturation in boiling distilled water, when cool the solution is poured off from the crystals and mixed to a thick paste with pure mercurous sulphate, which is again boiled to drive off any air; this paste is then poured on the surface of the mercury previously heated in a glass cell. A piece of pure zinc is then suspended in the paste and the vessel may be advantageously sealed up with paraffin wax.

Contact with the mercury is made by a platinum wire passing down a glass tube cemented to the side of the cell, and dipping below the surface of the mercury, or by a small external glass tube blown on to the cell and opening into it close to the bottom. A cell an inch in height is sufficient for all purposes, as it is not required to produce any sensible current. The mercurous sulphate (Hg_2 SO_4) can be obtained commercially, but should be *very carefully washed* in distilled water to free it from acid with which it is frequently contaminated, care should also be taken to obtain it free from mercuric sulphate (persulphate), the presence of which is known by the salt turning yellowish on the addition of water; the presence either of acid or persulphate would considerably affect the electromotive force.

Standard elements thus prepared are perfectly uniform in their electromotive force, and remain so certainly for 18 months, and probably for an indefinite time, provided the battery is not allowed to work.

Their value in absolute measure has been very carefully determined by a long series of measurements, and is found to be 1·457 volts or 145,700 absolute (metre-gramme) electromagnetic units.

We have here an arrangement † suited for measuring with the utmost directness and facility and in absolute measure, the value of the electromotive force of other batteries of all kinds, and also of earth currents and other manifestations of electromotive force, and also a

* (1872. No. 136.)

† An illustration is given of the arrangement of the Instruments and Connections.

means of comparing and recording their variations at different times and under different conditions.

The rapidity with which varying changes of electromotive force can be measured by this instrument is greater than with any other apparatus in use.

Scientific literature, both English and foreign, abounds with valuable researches, involving the value of electro-motive forces, the results being usually given either by comparison with a Daniell's cell, or a thermo-element, or frequently only by deflections of the instrument employed. As the Daniell is itself such a variable unit of comparison there is obviously a great want of precision in these results, and a great difficulty in comparing one result with another, a difficulty which would at once disappear if the writers would give their values in absolute measure.

There is one use of this instrument which I will now only allude to, although I think it one of considerable importance, that is in connection with the testing of telegraph lines. With an instrument of this kind fixed near the testing box of a telegraph office, it would be but the work of a few seconds to direct the operator at a distant station to hold down his key, and to measure absolutely the potential of the current passing through the receiving instrument, or by disconnecting that instrument the value of the potential at the near end of the insulated line.

This value depends on the resistance of the line, and the state of its insulation, and by comparison with the potential at the sending end we have all the information required by the electrician for practical use.

All the circuits of a large station may be thus tested in a few minutes, without any sensible interruption in the working. It is obvious that the same measurements may be made by the use of a condenser and standard element, and the author is of opinion that this system will eventually supplant the more slow and cumbrous method now in use of measuring the actual resistance of circuits.

As a specimen of the work the instrument is suitable to perform, I subjoin a few rough measurements of the electromotive force of some of the elements in ordinary use, expressed in absolute measure, and I hope some day to present to the Society a much fuller and more complete contribution on this subject.

Value of the Electromotive Force of Various Elements in B. A. Units.

		Volts.
1.	Daniell's pure zinc and pure saturated solution of sulphate of zinc with pure copper and saturated solution of copper * at 64° Fahr.	1·105
2.	Do. pure zinc and pure water with pure copper and pure saturated solution sulphate of copper ...	1·110
3.	Do. ordinary amalgamated zinc with solution (1 to 10) sulphuric acid and saturated solution sulphate copper †	1·09
4.	Ordinary commercial Daniell's set up with water ...	1·079
5.	Menotti's crystals of sulphate of copper resting on a copper plate, with a layer of sawdust and water supporting a zinc plate	1·068
6.	Ordinary zinc and copper with pure water	0·993
7.	Léclanché—zinc in saturated solution chloride ammonium, with mixture of peroxide of manganese and carbon	1·487
8.	Marie Davy—zinc water, with proto-sulphate mercury and carbon	1·527
9.	Grove's—zinc and dilute sulphuric acid with platinum and nitric acid	1·97
10.	Delaurier—zinc—and carbon with bichromate of potash and persalts of iron	2·249

Professor Foster said the advantages of the methods described must be obvious. One which might not strike most persons was, however, not mentioned. It was that the two batteries compared together by this method were really under the same conditions, whereas in Poggendorff's original method, of which this was a modification, the batteries were one working and one not working, so that if there was the smallest degree of polarization, which could not always be avoided, the comparison was not strictly accurate. The process described was capable also of giving convenient comparison between electro-motive forces, which differ more greatly than could be readily measured by Poggendorff's method. He had employed it for measuring the electro-motive force of joints of copper and iron with a Daniell cell. As the Chairman said the method only required to be more known to come into general use.

* This is a very uniform and constant form of Daniell's battery.

† It is believed that this is the standard form of Daniell's used in Sir Wm. Thomson's determinations.

Major Webber, R.E., said the Chairman had described this process as affording a means of measuring the potential or working current of lines in an office, and as likely to be substituted for the ordinary means of measuring the insulation of telegraph lines; and it seemed to him it might be interesting to many present this evening if the author would explain a little further its application in that respect to the case of the ordinary telegraph overhead lines in this country.

The Chairman said he should have great pleasure in complying with the request. It was, he said, the custom in all good telegraph offices to measure the resistance of the lines daily, or from time to time, to get an idea of their insulation and working condition. The method now adopted had this disadvantage, that it interrupted the circuit for a considerable time, and required disconnection at the distant station. He thought an electrician could obtain all he wanted by having fitted below the testing box an instrument of the character described, and by measuring the actual working potential of the current at the point where it entered the instrument. The clerk at the distant station is directed by a code signal to hold down his key for a certain time. The operator, by the side of this instrument, then measures the actual potential at the testing box of the current which was passing through the instrument, and that was the real test of the condition of the line. This method took account of two things—first, it showed whether the battery power at the other end was good, and secondly, whether the leakage on the line, or the resistance caused by badly soldered joints, was or was not so great as to reduce the potential down to less than proper working limits. With experience the operator would know the ordinary potential to be expected on any given circuit, and if it fell below that, something must be wrong, either the line was imperfect, or the force of the battery at the other end was too small. In the event of his instrument working badly, this momentary operation enabled the electrician to ascertain whether or not the line was in good working condition, if it were, the fault must be in his own instrument, if not, he would proceed by the usual method of obtaining direct resistances.

Mr. W. E. Ayrton said he had occasionally used this method in India, in measuring the potential at one end of the line, and simultaneously at the other end. It not only has the advantage of proving

whether the line is well insulated or not, but, testing between two stations, it showed not only the state of the line, but also which end of the line is worse insulated than the other, and, in that respect, this method certainly was exceedingly advantageous.

Captain HOME, R.E., said he had great pleasure in proposing a vote of thanks to Mr. Latimer Clark for taking the Chair, and for the two admirable papers he had read. The simplicity and excellence of the apparatus he had exhibited, he thought all would agree were only exceeded by the lucidity of the explanations he had given of the method of using it. He was not sufficiently acquainted with the subject to give an opinion upon the mode of testing proposed, at the same time, he perfectly understood it, which he should not have done, had it not been so clearly explained.

Mr. PHILLIPS begged to second the motion. He had listened with great pleasure to Mr. Latimer Clark's papers, and he wished that other leading men in the telegraph profession would condescend to address them in as clear and lucid a manner as Mr. Clark did. It was always a pleasure to listen to or to read anything from that gentleman, and there were young members, like himself, who would be delighted if other eminent electricians would explain and treat of electrical matters in the clear way in which they had heard them treated this evening.

The vote having been passed by acclamation,

THE CHAIRMAN said he was very much gratified by the way in which his papers and his services in the chair had been acknowledged, and he begged to return his best thanks for the honor they had done him.

The following Candidates were balloted for and declared duly elected:—

AS MEMBERS:—

G. L. Bristow, 1, Copthall Buildings.
J. Gutteres, Kingston, Jamaica.

As Associates:—

Thomas Angell, Buckingham Gate.
Thomas Blissett, India Government Telegraphs, Calcutta.
J. C. Fleming, Perth, Western Australia.
William Groves, 172, Great Portland Street.
Frederick Higgins, Exchange Telegraph Company, 17, Cornhill, E.C.
Sidney James, Post Office Telegraphs, Telegraph Street, E.C.
W. F. King, Hooper's Telegraph Works, Millwall.
Joseph May, 2, Trinity Cottages, Church Street, East Greenwich.
F. G. Ticehurst, Battle, Sussex.
John G. Uren, Post Office, Penzance.

As a Student:—

Thomas Gatehouse, 172, Great Portland Street.

The Meeting then adjourned.

The Twelfth Ordinary General Meeting was held on Wednesday, February 12th, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

THE CHAIRMAN said he had to make an announcement which he was sure would be received with much satisfaction by the members. He was happy to state that they had received two very handsome presents from their members. Mr. C. F. Tietgen, the Chairman of the Great Northern Telegraph Company, Copenhagen, had presented £100 to the funds, in addition to £25 by which he became a life member; and Mr. H. G. Erichsen, of the same company, had presented the like sum, and had also become a life member by the payment of £25.

Professor FORSTER rose and said, he was sure he only expressed the feeling of all the members of the Society in proposing a cordial vote of thanks to Mr. Tietgen and Mr. Erichsen for their very liberal donations to the Society. The generosity of the gift was all the more striking, inasmuch as the Society was so much in its infancy that they must consider it somewhat a venture to become a life member of it; and they had still to prove that they were worthy of being joined as a life-long affair. This was an instance of great liberality on the part of these gentlemen, who instead of waiting to see what this Society eventually became, had given those handsome donations.

Major WEBBER begged permission to second the motion. As their treasurer he felt how much the Society was likely to benefit, not only by the example but by the actual gift, it was not sufficient for him to say.

Mr. ERICHSEN, in acknowledgment of the compliment paid him, said—"I believe in Telegraphy."

THE ACTION OF LIGHT ON SELENIUM.

The following communication from Mr. WILLOUGHBY SMITH was then read :—

"Wharf Road,

"4th *February*, 1873.

"My dear Latimer Clark,—Being desirous of obtaining a more suitable high resistance for use at the shore station in connection

with my system of testing and signalling during the submersion of long submarine cables, I was induced to experiment with bars of selenium—a known metal of very high resistance. I obtained several bars, varying in length from 5 to 10 centimetres, and of a diameter from 1 to 1½ millimetres. Each bar was hermetically sealed in a glass tube, and a platinum wire projected from each end for the purpose of connection.

"The early experiments did not place the selenium in a very favourable light for the purpose required, for although the resistance was all that could be desired—some of the bars giving 1,400 megohms absolute—yet there was a great discrepancy in the tests, and seldom did different operators obtain the same result. While investigating the cause of such great differences in the resistances of the bars, it was found that the resistance altered materially according to the intensity of light to which they were subjected. When the bars were fixed in a box with a sliding cover, so as to exclude all light, their resistance was at its highest, and remained very constant, fulfilling all the conditions necessary to my requirements; but immediately the cover of the box was removed, the conductivity increased from 15 to 100 per cent., according to the intensity of the light falling on the bar. Merely intercepting the light by passing the hand before an ordinary gas-burner, placed several feet from the bar, increased the resistance from 15 to 20 per cent. If the light be intercepted by glass of various colours, the resistance varies according to the amount of light passing through the glass.

"To ensure that temperature was in no way affecting the experiments, one of the bars was placed in a trough of water so that there was about an inch of water for the light to pass through, but the results were the same; and when a strong light from the ignition of a narrow band of magnesium was held about 9 inches above the water the resistance immediately fell more than two-thirds, returning to its normal condition immediately the light was extinguished.

"I am sorry I shall not be able to attend the meeting of the Society of Telegraph Engineers to-morrow evening. If, however, you think this communication of sufficient interest, perhaps you will bring it before the meeting. I hope before the close of the session that I shall have an opportunity of bringing the subject more fully before the Society in the shape of a paper, when I shall be better able to give

them full particulars of the results of the experiments which we have made during the last nine months.

"I remain, yours faithfully,
"WILLOUGHBY SMITH.

"Latimer Clark, Esq., C.E."

THE CHAIRMAN remarked that he thought this was a very interesting scientific discovery, and one on which it was probable they would hear a good deal in future. He had himself witnessed these experiments, and could confirm all that Mr. Smith had stated. Its sensibility to light was extraordinary, that of a mere lucifer match being sufficient to effect its conductive powers. For the experiments which would be carried on in future, selenium, sulphur, and phosphorus, which belonged to the same group, would be experimented upon, as also, he believed, tellurium. He had heard of one instance in which plumbago had shown similar effects. Selenium, he said, existed in two forms, like sulphur and phosphorus. In the ordinary form, vitreous, it resembled dark brown glass, and when softened by heat it could be drawn into threads at a temperature twice that of boiling water; but when the heat was carried beyond that it became crystallized, and it was only in that state the material became conductive. Mr. Smith showed him some experiments, in which he placed pieces of rock salt, alum, and other substances before the selenium, which might have intercepted the rays of heat; but the effect was as powerful through these as through the ordinary air. It was satisfactory to know that Mr. Smith was continuing his experiments in this direction, and he had no doubt his future communications on the subject would be as interesting as the present one had been. It seemed to him (the chairman) to afford a most reliable means of measuring the intensity of light, and to constitute a perfect photometer.

The first Paper read was "ON THE APPLICATION OF IRON TO TELEGRAPH POLES," by Major Webber, R.E.

THE use of iron in telegraph poles is generally supposed to be a matter of £ s. d. Iron has been so employed for many years, and numerous modes of adapting the material to the requirements of a standard to support telegraph wires have been from time to time

suggested. There have been wrought iron and cast iron poles, poles composed of both natures of iron, and poles in which iron and wood have been combined.

The past history of iron telegraph poles seems to me to shew that as the necessity arose, and the immediate economy of using iron could be proved, each engineer devised a pattern which he thought would answer the requirements of the situation. Thus, when great lines came to be erected in India, Central Asia, and Australia, about which, no doubt, many here present to-night know much more than I do, iron poles were specially made, which, when erected answered all ordinary constructional requirements, and have since been proving themselves to be the most economical means of carrying wires above ground.

Their advantages in such countries, particularly the two latter, are clear, because the iron can be used in the smallest quantity requisite to carry the weight, and the additional strength required to withstand strain can almost always be obtained by lateral support—or, in other words, lines can be built according to true principles of construction, and the engineer has only to provide the requisite quantity of iron to insure rigidity and steadiness of support for his wire at a given height above the ground.

Knowing then the difficulties of transport, labor, and maintenance, in countries with scanty population, and comparing the life of wood with that of iron, a very wide margin of result in favour of the employment of the latter material is evident.

We may next consider the comparative advantages of the use of iron and wood on railway lines of telegraph, in countries where the cost of creosoted timber and iron delivered at a station on the line would be nearly the same as it is here.

In building lines of telegraph along the side of a line of railway, the engineer may be said *also* to have it in his power to dispose his material in accordance with true principles of construction, (certain parts of the line excepted,) and with him there need be no other consideration than to make his pole sufficiently strong to carry the weight of wire steadily; and, against strain, pressure of wind, and other forces, he has ample space to apply lateral support.

But, when a line of telegraph has to be built along a road in a country like the United Kingdom, where private and vested rights over-ride all public considerations, the engineer must lay aside all

thoughts of placing his material to the best advantage, with reference to a minimum safe use of it.

I am sure it is the experience of every telegraph engineer in England, who in past years has had to deal with the numerous propositions that have been made suggesting the use of iron poles, that, while acknowledging the advantages of that material, he has found himself unable to adopt the designs because they have been made with a view to being generally applicable to all situations. While, what he has required has been, a variety of forms of a good pattern of pole, so as to meet every necessity of construction. Even if he had not had to meet financial considerations, which do not appear as yet to have shown an economy in the use of the more durable material even this kingdom, where that material is relatively cheapest.

The experience of building lines of telegraph along the roads of such a country as the United Kingdom shows that an endless variety of conditions affect the considerations of the engineer in constructing and placing his poles, but the main features of these conditions are due to the form of the line which the road follows. The mathematically correct curves of the railway are not to be met with. But every shape is encountered, *from* the straight line and abrupt angular change of direction to another straight line, *to* the extremes of tortuous and unnameable curves.

The road telegraph builder, therefore, has to keep in view two distinct forms of construction, the one a series of straight lines with angular changes of direction, the other a similar series with curved changes of direction of various radii, combined in each case with continuous and irregular curves. In other words he changes direction either by curves or angles.

Now it is this view of the case which materially influences the application of iron to telegraph poles as a material. If you have the easy circumstances of railway construction to meet, the uniformity of the curves, and almost unlimited space for support against strain afforded by railway roads, clearly defines how your iron should be disposed to the best advantage. But when you have all the difficulties of road construction to contend with, experience seems to point out that straight lines and angles enable you to distribute a costly pole material in the most economical way.

This entails the use of a pole, which is sufficiently strong to carry the weight of the wire at the requisite height from the ground without

bending, and rigid enough to resist the pressure of wind. When placed in the straight this pole will require no side support, but when at an angle it must not be expected to resist any of the lateral strain due to the wires. If space for side support could always be obtained, there would be no difficulty in so adjusting the struts and stays as to give them the whole duty of sustaining all the lateral wire strain along the most tortuous and irregular line of road, in which case iron could be as economically used in a road pole as in a railway pole, necessary difference of height excepted.

But this side support not being everywhere obtainable, in sites secure enough to allow of the whole strain being so borne, a fresh feature in poles presents itself—namely, it becomes necessary to make poles which will carry the strain of wires, with no other means of resistance than that afforded by the ground in which they are placed.

When a telegraph constructor has to place a *wooden* pole under such circumstances, he has to double his pole or truss it if possible; but, unless he can fix the pole at such an angle to the ground as that the strain is partially converted into thrust, and in a sufficient degree to prevent the pole bending, he cannot find one of wood that will long resist the enormous strain to which a pole is subject when placed as I have described, when carrying a large number of wires.

In many situations the placing of poles out of the perpendicular is inadmissible, and it remains to be considered if iron can be got to do what wood cannot.

But first it will be as well to examine the statical conditions of a telegraph pole so placed. We shall do so as if it was a beam fixed at one end and weighted at the other.

The strain of 13 No. 8 wires on a pole placed at a contained angle of 120°, the wires being pulled up so that the sag is 2 feet in a span of 90 yards, would be about 400 lbs from each wire in the direction of the line bisecting the angle. Placing the wires one on the saddle and two on each arm one foot apart, according to the Postal Telegraph pattern, the strain of the 13 wires will be equal to more than 2¼ tons applied at the end of the pole.

Allowing for such a pole a length of 32 feet, 6 of which are in the ground. We have to deal with the case of a lever arm 26 feet long, of a weight 2¼ tons, resulting in a moment on the section of fixture, equal to 9 tons, leaving out the weight of the pole.

Multiplying the section of fracture under this weight by three, as the lowest co-efficient of safety, we arrive at the following section for columnar poles, viz., 2 feet external diameter, and more than 6 inches thickness of metal.

This represents a very large mass of metal, and this one simple example is enough to give some general idea of the conditions which have to be met.

The next point after its absolute strength that may be considered, is the means of fixing a pole, so that the mass which holds the pole will be capable of resisting the pressure to which it is subject.

Continuing the case already described, we should find that there is a centre about which this lever turns, which will be somewhere in the length below the ground, the position of which will vary with the comparative resistance of the material which holds the pole, at the bottom of the hole into which the pole is inserted, and at the surface of the ground.

Assuming this to be four feet below the ground line, we discover that the pressure of the pole against the surrounding soil in one direction is equal to 7½ tons at the surface, and such being the case, it can readily be understood that here is the real difficulty which has to be encountered. In fact, that it is not difficult to make a pole with the necessary strength, but that it is very difficult to obtain a sufficiently unyielding mass to hold it upright against the horizontal strain to which it is subject.

When such forces as I have described have to be dealt with, I have doubted the expediency of proposals for iron poles which adopt the plan of terminating the lower extremity with a base plate.

It can be seen at a glance, that, unless the plate is so large as to carry on it a weight of material greater than can be lifted by the strain, such a plate is a disadvantage, as its use necessitates large excavation, and entails the disturbance of the material on whose resistance to direct pressure so much depends.

In the one case, with the plate, the stability depends on the weight of the mass holding the plate down—in the other, on the power of resistance to horizontal pressure of the soil.

In each case, it may be said, that it is only a question of the depth to which a pole is inserted in the ground, but the difference in quantity of soil to be excavated is large, and the cost of replacing it in a compact form increases with the mass disturbed.

The force already described, applied in one direction at the top of a pole of the length before named, and having a base plate 2 ft. 6 in. diameter, would have the effect of turning it about a point in the centre of the edge of the plate on the same side as that on which the force is applied, the result being, to lift the other edge with an upward pressure against the superincumbent mass equal to 25 tons. Doubtless it will be answered, that the plate is only intended to do a small portion of the work, and the surrounding earth will do the rest; but unfortunately the existence of the plate, which is itself in the worst position to take the work, to a great extent neutralises the resisting power of the surrounding mass.

Again, the existence of the plate at the base requires a continuance of the increase of sectional strength down to the plate, in proportion to the work the plate is expected to perform, whereas a pole which depends only on the resisting power of the mass in which it is placed against horizontal pressure, can be made strongest at a point above the neutral axis, by keeping that axis low, thus effecting a saving in metal.

I *may* be of course met by the objections that base plates were never intended to do the work I have described, but are only of use in marshy soil, or in ground of an exceptional nature. But if plates are only required for such services, why attach them to poles which are evidently required to do more work than merely to carry the incumbent weight of the wires?

Whatever may be the result of the enquiries which the Engineer-in-Chief of the Post Office has instituted as to the pole of the future, it appears evident, that to economise material, various classes of pole must be available in constructing such lines, as it is required should be erected, with iron instead of wooden poles.

There has been much to admire in the various forms hitherto proposed.

There have been telegraph poles constructed like railway signal posts.

Poles like split tapered tubes, made with cast iron, and having diaphragms at intervals inside, to be used singly and together.

Poles of cast and wrought iron tubes connected telescopically.

Poles of cast and wrought iron tapered tubes, connected by socket joints.

Various forms of cast iron bases, with wrought iron tapered tube tops.

Lattice and Riband poles.

Poles of multiple bars, and poles of T and H iron sections.

The construction of telegraph lines with these poles varies in cost from £50 to £250 per mile according to the nature of the route and the number of wires.

The inventors of these poles, from some of the specifications I have seen, have, in most cases, very much misunderstood the conditions of the work which a telegraph pole may have to fulfil.

There is one significant fact, however, as regards the use of iron in the United Kingdom, that, when in 1865 some experience had been gained abroad, and iron telegraph pole makers were pushing their manufactures in the telegraph world, none of the great English Companies had as yet seen their way to its use.

While, as I have said, a thorough investigation of this subject is being conducted by the Post Office, it would be premature for me to go further into the question, or to describe my own views on what is required; but there is one point upon which I think this Society would feel much interest in eliciting information, and that is, the practical experience of those Engineers who have used iron sockets in the ground to carry wooden telegraph poles. Various sorts have been proposed from time to time, and there can be no doubt that an economy might be effected in this direction, if it was found that steadiness could be obtained, and that the decay of the wood at the joint could be prevented.

The second Paper read was "ON TELEGRAPH POLES," by Lieut. Jekyll, R.E.

THE subject of Telegraph Poles is one necessarily so familiar to this Society, that I cannot hope to bring forward any information not already known to practical men. But it may not be unprofitable to regard certain practices in connection with their use from a theoretical point of view, analysing the forces and strains to which they are subject, and then applying to practice, the results of the theoretical analysis. I am unable to discover that this has yet been done in the various text-books which treat of practical telegraphy, except in that of Blavière, whose treatment of the subject is, however, somewhat superficial and empirical. The investigation is not without interest, as it confirms in every particular the practice which has been arrived at by experience in the field.

I propose to place before you the calculations which reveal the nature of these strains, and to do so fully, as none but those of a most elementary character are needed for their determination.

The whole subject of telegraph poles is too wide for the scope of a single paper; I shall therefore confine myself to one special branch, viz., the means used for supporting and strengthening poles, which are subject to lateral strain, such as occurs at every angle in a line of telegraph.

It will first be necessary to analyze the strain put by the wire upon the pole itself, and then the counteracting strain of the means of support, deducing therefrom the best manner of applying the support so as to neutralize the forces to which the pole is subject.

Fig. 1.

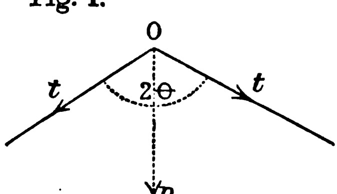


Fig. 1 represents an angle in plan, O being the pole. Let t be the tension of the wire and 2θ the horizontal angle. (It is assumed for the present that a single wire only is dealt with, and that it acts directly on the pole itself.)

Then, plainly, the effect of the tensions $t\,t$ at O will be equivalent to $2t \operatorname{Cos} \theta = p$ acting in the line Op which bisects the angle. This,

then, will be the horizontal strain on the pole, but in addition to this there will be the weight of the wire—the pole has to bear half the weight of the span on each side of it. This force, which we will call ω, acts vertically downwards, the horizontal force, for brevity called p, acts at right angles to it, and the two combined produce a resultant P, equal to $\sqrt{p^2 + \omega^2}$

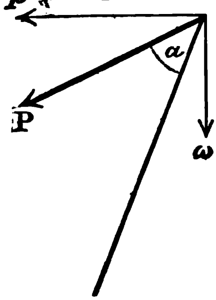


Fig. 2.

Fig. 2. Suppose the direction of this force to make with the pole an angle α; then resolving P along the pole, and at right angles to it, we get—

P Cos α acting down the pole, and
P Sin α „ at right angles to it.

The former is expended in pressing the pole into the ground, and so becomes harmless. The latter is the effective part of the strain, and is the force which we require to neutralize to keep the pole at rest. Substituting the value of p and P found above, the force is represented by Sin α $(4\ t^2 \text{ Cos}^2\ \theta + \omega^2)^{\frac{1}{2}}$, from which, all the quantities being known, we may obtain by substitution the force in pounds in any given example.

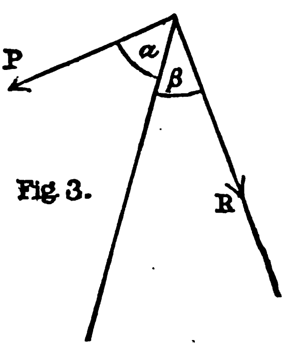


Fig. 3.

Fig. 3. Now suppose a stay be fixed to the pole at the same point as the wire, and make an angle β with the pole. The reaction of the stay will be split up as was the force of the wire, and if R represent the whole tension

R Cos β will act down the pole, and
R Sin β „ at right angles to it.

The former will combine with the corresponding component of the wire strain to press the pole into the ground, while the latter, which is the useful portion, counteracts the effective part of that strain, hence—

$$\text{R Sin }\beta = \text{P Sin }\alpha$$

$$\text{and R} = \text{P}\,\frac{\text{Sin }\alpha}{\text{Sin }\beta}$$

from which equation R may be calculated, and so the strength of a stay for any given situation may be found. It is also apparent that as the angle β increases, its sine increases, and consequently R, the strain of the stay, decreases; and this points to two practical conclusions, viz., that the poles should not be set upright, but inclined, and that the stay should have as wide a base as possible, consistent with practical considerations. It is further apparent, that by inclining the pole the angle a is diminished, and as we have seen that the destructive part of the wire strain is P Sin a, it follows that this strain is also diminished.

It would be possible to calculate the exact angle at which a pole should be set and the angle of the stay, so as to secure the best possible position in a given case; but such a calculation would involve a complicated maxima and minima problem of the differential calculus, and would not be of practical utility.

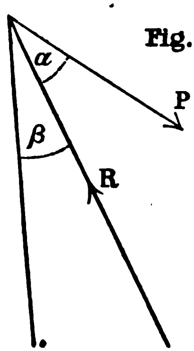


Fig. 4.

Fig. 4. In the case of a strut, the effective portion of the wire strain will remain as before P Sin a, and the reation R of the strut can be resolved into R Cos β acting up the pole, and R Sin β at right angles to it, the latter being the sustaining element of the thrust.

Then, as before, in a state of equilibrium, $R = P \frac{\text{Sin } a}{\text{Sin } \beta}$, but two important points of difference are to be observed between the nature of the support afforded by a strut and a stay, showing the theoretical superiority of the latter. In the case of the stay, the element of the strain which acts along the pole is exerted downwards, the result being to hold the pole firmly in the ground; while in the case of the strut, the corresponding element acts upwards, and therefore tends to lift the pole out.

Secondly, the angle β can seldom be large for practical reasons, and consequently the chief portion of the reaction of the strut is expended in this useless, or worse than useless manner, a small proportion only being available to balance the strain of the wire. Whence greater strength is required in a strut than would be wanted in a stay calculated to do the same work.

As before, it is equally necessary to incline the pole against the strain, so as to reduce the angle and diminish the amount of destructive force of the wire. Another cause of superiority of the stay, is that the strain upon it being a tension, the whole strength of the material is available, whereas, the strain upon a strut being compression, it is liable to buckle before the whole power of resistance to crushing can be brought into play.

It is of course assumed in this comparison, that the stay has been anchored sufficiently securely so as not to draw, a species of failure to which struts are not liable.

I venture to express an opinion that this theoretical superiority is borne out in practice, and that it is in general advisable to use stays in preference to struts in situations which admit of their employment.

We have hitherto dealt with the question on the hypothesis that the support was applied to the pole at the same point as the wire. But this is seldom the case in practice, and we will now proceed to ascertain how the strains are modified when these two points do not coincide.

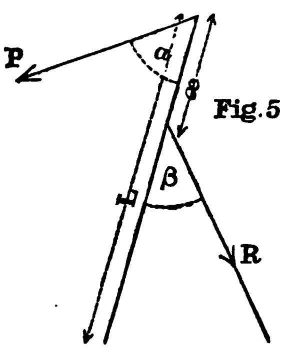


Fig. 5.

Fig. 5. Let x be the distance between them, and L the length of the pole above ground, to the point of attachment of the wire. Then, as before,

P Sin α is the effective strain of the wire,

R Sin β is the effective strain of the stay,

but, as they act at different points, we must consider the effect of their moment, which gives us for equilibrium,

$$L P \operatorname{Sin} \alpha = (L - x) R \operatorname{Sin} \beta$$

$$R = \frac{L (P \operatorname{Sin} \alpha)}{(L - x)(\operatorname{Sin} \beta)}$$

from which it is evident that as

$(L - x)$ is less than L,

R must be greater than $P \dfrac{\operatorname{Sin} \alpha}{\operatorname{Sin} \beta}$

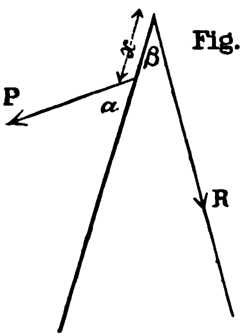
hence, we see, that the greater the distance between the point of attachment of the stay and that of the wire, the greater will be the

strain upon the stay, and, consequently the strength which it is necessary to give it, so long as the stay is fixed below the wire. If, on the other hand, the stay is fixed above the wire,

$$R \text{ will be less than } P\,\frac{\operatorname{Sin} \alpha}{\operatorname{Sin} \beta}$$

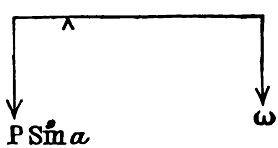
and then the tension will diminish as the distance increases, though the length of the stay will have to be increased.

Fig. 6.



A more important consequence remains to be considered. When P and R act at the same point, their resolved elements in the direction at right angles to the pole neutralize each other, and the pole remains at rest. But when they do not, they cannot neutralise each other, though their effects may be equal, for the pole becomes a lever of the first order, of which the point of attachment of the stay is the fulcrum, P Sin α the power, and the reaction of the ground the weight which we may call W.

Fig. 7.



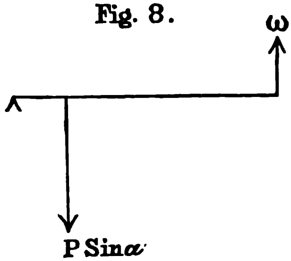
Here two forces are introduced,

x P Sin α to bend the pole above the stay,

$(L - x)\, w$ to bend the pole below the stay.

Fig. 8. While moreover the pole has a tendency to break at the stay attachment.

Fig. 8.



If the stay be attached above the wire, the lever is of the third order, and the bending force is diminished as the point of application of the force approaches the fulcrum.

To apply these results practically, we see that when a stay is not attached to the same point on the

pole as the wire, the pole will be subjected to forces having a tendency to bend and break it, a stronger or a longer stay than would otherwise be necessary will be required, and there will be a reaction of the ground at the foot, which will have to be met by the use of blocks of wood or stone.

Theory then points out conclusively the best place for the attachment of stays and struts, and also shows that if for any reason that place cannot be made use of, it is better to attach a stay or strut above than below the wire.

We have hitherto considered the strain of a single wire only. A number of wires may be treated in a similar manner by supposing their combined strain to act as a single resultant. We may find the point on a pole where this resultant may be supposed to take effect by a process analogous to that of finding the centre of gravity of a number of equal bodies, when the wires are all of one gauge, and if otherwise, by assuming the weights of the bodies to be proportional to the sectional areas of the wires. Where, as is usually the case, the wires are arranged symmetrically and are all of the same gauge, the resultant force may be assumed to act at the middle point of the group. When the number of arms is odd, this point will be near the bolt of the centre arm, when even, near that of the upper centre arm. At any rate, these points will be found sufficiently near for practical purposes.

It would be easy to fix an iron loop to the arm bolt, as a means of attaching the stay, the stay being led between the wires, and the clearest interval being found by stretching a line from the loop to the ground.

The loop if used, should be put on inside the arm, and not so as to embrace both arm and pole, as in this position, it has a tendency to bend the bolt and cant the arm. It should be galvanized like the bolt and wire. One advantage of its employment among many would be the facility with which the stay could be moved from one place on the pole to another, should the number of wires be increased, and so vary its support to meet the altered condition of the tension.

When on account of the number of wires being large and the angle sharp, it is advisable to employ two stays, the best positions can be readily found by considering the wires in two distinct equal groups, finding the centre of force of each, and applying one stay as nearly as possible to each centre. If instead of this, one stay were

placed at the top of the pole and the other nearest the lowest arm, the pole would bend between them.

Struts are more troublesome and difficult to manage than stays. If applied to the pole just at or under the lowest arm the strut is sure to cause it to bend both above and below the point of attachment, and unless the base of the strut be at a good distance from that of the pole will tend to prize the latter out of the ground. These defects are sometimes remedied by anchoring or foot staying the pole, and by bracing the pole and strut together in the middle with twisted wire, which precaution, however, does not prevent the top from bending.

Adopting the principle already explained, the strut should be applied to that point on the pole at which the forces act, when the tendency to bend and the power of lifting are both got rid of; and, as in the case of stays, if that point cannot be made use of, it is better to place the strut above than below it.

In some cases want of strutting room would cause a strut so fixed to interfere with the wires, but the difficulties so arising could be easily overcome by using arms all of one length, or in extreme cases by shackling.

The strut might be carried up as far as, or just above the middle arm, and scarfed to a depth of not more than half its thickness. Where it passes the lower arms, it would have to be notched to the same depth as the pole itself, and might be bolted with a common pole bolt to each arm. Two 6″ spikes and 2 turns of hoop iron embracing the top of the strut and pole would be sufficient to complete the attachment of the two.

A strut should not be fixed to a pole by bolting the two together, as to do this a hole must be bored through the pole, which weakens it at a critical point.

The greatest danger, as far as I am aware, of fixing struts and stays as described above, is spiders; but it is probable that means might be found of coping with them should they really prove a source of trouble.

The cases which offer the greatest difficulties to the constructor are those of poles where, while support of some kind is absolutely necessary (or at least very desirable to save an excessive expenditure of timber), the ordinary means, mentioned above, are precluded by local circumstances. In such an instance, the best artifice is probably trussing. The principle of a trussed beam is very familiar to engineers, and it can be easily applied to stiffen a telegraph pole in

the following manner. I venture to give details of the operation, as I believe it is not described in the text-books:—

The pole is supported horizontally by a crutch at each end, in which position it sags slightly in the middle. A block of wood about 2 ft. in length and the same thickness as the pole, is halved into and spiked to the butt of the pole at right angles to its length. A spur about 1 ft. in length is fixed on the side of the pole, half way between the ground line and the top. An ordinary stay rod is grooved into the side of the butt, and passed through the block at the bottom, and an iron loop is attached to the bolt of the top arm. A twisted wire stay is then made between the loop at the top and the eye of the stay rod at the bottom, which when tightly secured is stretched over the spur as a bow is drawn. It will then be found to be quite tight and to impart a slight bend to the pole, which can be further increased if required, by screwing up the nut at the end of the stay rod.

I have found in practice a four-armed spur to be the best shape, as it is safe from bending or collapsing in any direction, and cannot be knocked off. The block at the bottom would perhaps be better if bolted side by side to the foot of the pole, instead of at right angles to it, though I have never known the other arrangement fail.

Great care has to be taken in setting the pole to get the truss wire in the same vertical plane with the resultant of the wire strains, otherwise the truss becomes inoperative. As a pole so trussed is merely stiffened and enabled to resist bending, but is not in any way supported, the whole support required has to be furnished by the resistance of the ground to pressure; and it is therefore important that the pole should have some firm, hard substance, such as a large block of wood or stone, to oppose its thrust at the ground line, and a similar block at the foot of the pole on the opposite side.

Fig. 9.

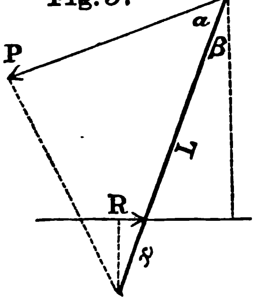


Fig. 9. Suppose the depth of the pole in the ground $= x$, and the length of it between the ground line and centre of wire streams $= L$, let P be resultant of wire strains making the angle a with the pole and β the inclination of the pole to the vertical; then taking moments about the foot of the pole, and assuming the resistance at the ground line $=$ R, we get—

$$P\,(L + x)\ \mathrm{Sin}\ \alpha = R\,x\ \mathrm{Cos}\ \beta$$

$$\text{and } R = P\,\frac{(L + x)\ \mathrm{Sin}\ \alpha}{x\ \mathrm{Cos}\ \beta}$$

from which the reaction at the ground line can be calculated, and the required strength at that point to meet the pressure of the pole.

Sometimes double poles or A poles are made use of to withstand an exceptional pressure. The latter depend for their stability upon the width of the base, and the forces acting upon them have to be calculated and considered in a way quite different from that which will enable us to ascertain the strain upon ordinary strutted and stayed poles. I shall, therefore, not enter upon this subject, as a dissertation on braced poles would be sufficient for a paper of its own. There are also collateral branches, such as ground staying and the pressure of wind, which I forbear to touch upon.

I beg to remark in conclusion that, though theoretical can never take the place of practical knowledge, it certainly assists it. In the present case, the amount of mathematical knowledge required is so small, that any intelligent man in charge of a working party might work out for himself the strains in exceptional cases, and apply his materials in the most economical manner instead of working wholly by rule of thumb, as is sometimes the case.

The third Paper read was "ON IRON TELEGRAPH POLES," by Mr. C. W. Siemens, F.R.S., D.C.L.

THE construction of an iron telegraph pole has occupied my attention for many years. The object was to combine lightness and convenience of construction with the attainment of a maximum of strength and resisting power to sudden jerks, and to oxydation. This consideration led me to abandon the ordinary mode of fastening poles by setting considerable lengths of them underground, and to the adoption in its stead of a buckled wrought iron plate, which combines very great rigidity with a certain toughness, enabling it to yield to sudden and excessive strains.

It is evident that a straight pole which has been fastened into the ground by only burying a part of it will, if once shaken by some sudden jerk, never attain its former firmness again, whereas a pole which is fastened to a plate has always the whole weight of earth resting on its foot-place to keep it steady even if it should be shaken by any temporary cause. The portion of the post which is partly buried in ground, and, therefore, exposed to the simultaneous action of moisture and air is made of cast iron, and is of a tubular form. This tube is fastened to the buckled plate by means of four bolts, and is provided at its upper end with a suitable socket to receive the upper tube The latter, which forms the principal part above ground is made of wrought iron. The shape which has been adopted for it is approximately a parabolic one, that is to say, the tube is for about 2 feet cylindrical, thence tapering off towards the top. By these means a distribution of metal is obtained, which, with a minimum expenditure of material, gives a maximum amount of rigidity.

The proportion of the diameter of the tube to the thickness of metal is such, that a horizontal strain just establishes a balance between the tendency of collapsing or flattening of the tube and that of breaking it. A tube, therefore, of the same weight per foot, but of a larger diameter, would collapse; whereas if the diameter was to be decreased and the thickness of the metal to be increased proportionately, the tube would break. It is also important to observe that the

metal of the conical tube is of sufficient thickness to resist the action of oxydation for an indefinite length of time, it being a well established fact, that a thin plate of say an eighth of an inch thickness rusts through in much less than half the time than another of a quarter of an inch thickness.

The manufacture of these tubes presented practical difficulties at first which were overcome by Mr. Brown, the manager of Messrs. Russell, with the aid of a furnace specially contrived by me for that purpose.

The upper tube is usually cemented into the socket of the cast iron pedestal tube by pouring into the annular space, between the two tubes, a fused cement, consisting of a mixture of sulphur and oxide of iron, which upon congealing sets extremely hard. Lately my firm have also adopted a method of setting the iron tube with an inverted conical end into a conical socket at the upper extremity of the cast iron tube.

The iron posts above described were first erected by my firm in Spain, South Africa, and other places, in the year 1863, and have remained in perfect working condition ever since. Since 1863 upwards of 180,000 of these posts, representing more than 9,000 miles of telegraph line, have been erected in New Zealand, Ceylon, Egypt, India, Persia, Russia, Mexico, Brazil, River Plate, Chili, and other parts of South America, with the same satisfactory results.

The height and dimensions of these posts vary according to circumstances. If only one or two wires are to be carried and cheapness is an object, posts of a total length of 19 feet 8 inches are used, standing 17 feet above ground when erected, it being usual to place the post 2 feet 8 inches in the ground. The total weight of this post is 184 lbs., and as it can be carried in three separate parts the weight of the heaviest one will be less than 100 lbs. Such a post will support a dead weight of 560 lbs. suspended horizontally from its upper extremity over a pully, without breaking. In other cases posts which weigh 254 lbs., and bear a testing strain of 900 lbs., have been adopted. At all points where the line is exposed to an extraordinary strain, heavier poles are introduced, and usually for the first mentioned kind poles which weigh 295 lbs., and bear a strain of 1,120 lbs., and in the latter case poles of a weight of 340 lbs. and 1,350 lbs. breaking strain. The cost of these iron posts has varied from 22s. 6d. to £3 16s., according to their dimensions, and the fluctuating price of iron.

As a rule, they may be taken to be from two to three times dearer

than ordinary wooden posts of the same strength. In many countries, however, where both timber and iron posts would have to be carried over great distances, and by such means of transport as are available in half-civilized countries, iron becomes as cheap as wooden posts at the point of erection, owing to their lesser weight and the facility of transport resulting from their being carried in pieces of convenient weight and bulk.

But, considering their greater durability and consequently reduced cost of maintenance, they would be, I believe, the cheaper material even in this country.

In southern countries, where wood is subject to dry rot, and where wooden posts have to be renewed every two or three years, the relative advantages of iron posts of this description have proved to be very great indeed.

The fourth Paper read was "ON THE RIBAND TELEGRAPH POST," by Robert Bristow Lee, Assoc. Soc. T.E.

MUCH interest having been shewn by Members of the Society, and by other persons, relative to the manufacture and the special advantages of the iron pole known as the Riband Telegraph Pole, the author has thought it advisable to bring the matter before the Members of the Society in a short paper, in order that they may be able to judge of the merits and demerits of the telegraph pole, which so essentially differs from all others in appearance and in construction.

The pole, as may be seen from the specimens produced, consists of (1) a number of straight angle irons, varying in number, according to the height and diameter of the pole, (2) surrounded by a series of wrought iron ribands (in reverse directions), riveted at each crossing in themselves and also with the angle irons, and (3) a circular base-plate with a tripod to which the pole itself is firmly and securely attached.

MANUFACTURE.

The method of manufacture may be briefly described. A hollow mandrill, so constructed as to allow of its collapsing or occupying a less diameter than when fixed in its proper quality of a mandrill, is made according to the size and requirement of any specified pole, either purely cylindrical or tapering, or tapering as is the case with a telegraph pole, the exterior of the mandrill is grooved in a special form with the required number of grooves from right to left, at equi-distances. An equal number of grooves are made from left to right, so that these grooves intersect each other at regular intervals, and form, so to speak, diamonds of a size slightly decreasing upwards with the taper of the poles, these grooves form the receptacle for the wrought iron ribands which are wound on by machinery in a very perfect and careful way, compensation being used in the machine for the decreasing diameter of the pole.

The ribands are wound on, one after the other, in the first place those from right to left from the base upwards and their several ends secured.

The second series are wound on in the reverse direction, from left to right, but starting from the top and finishing at the base, the ends of these are secured, the second series, consequently, externally overlap the first series, and they are bolted together. This is done with ease, for each riband before being wound has holes punched in it on a template at regular and stated intervals, so that when the ribands are wound on and overlap each other, the punched holes exactly coincide, and a space being allowed in the mandrill, the temporary process of bolting becomes easy.

The packing of the mandrill is removed, so that it collapses, and allows the pole to be withdrawn with ease on to a cylindrical or tapered bar.

The angle irons are then inserted *seriatim*, and placed in their proper positions equi-distant. Each iron is previously punched with holes at distances corresponding to the intersections of the ribands; at these points the bolts are removed, and the ribands and angle-irons securely riveted. After each angle-iron has been securely riveted, the remaining intersections of the ribands are riveted.

The pole, therefore, in this state remains complete, with the exception of the cap and its base piece. The cap is made to fit inside the top, to which it is firmly secured, and it may consist of wood or of cast iron, whichever may be considered more desirable.

The base consists, in some cases, of a cylindrical foot and base plates, but in the sample produced of a circular plate, to which are attached three cast-iron legs of strong metal, and of a peculiar form. They are firmly secured to the plate by bolts through projecting feet; this portion of the pole is made separately, and is so transported with the poles to wherever they may be required; the legs are so grooved that the angle-irons fit compactly within them by pushing the base upwards inside the pole to the required height. In the interior and between the legs are pieces of metal (forming part of the legs) of a concave form; fitting against these are two solid circular plates connected by a bolt with a double thread; the screwing up of this bolt lowers the one and heightens the other, and acting against the curved surfaces, they tend to secure the legs of the base to the angle-iron in a very firm manner.

Whilst the ribands terminate at or just below the ground, the tripod is carried some distance above the ground.

The poles when complete are painted or galvanized, the latter

process is accomplished with much success, and, considering the advantages derived, at no very great cost.

It was originally intended arming the posts with wrought iron brackets, but it has been found in practice that the ordinary oak arm answers better. The arm is placed against one of the diamonds in the post and secured by a clip round the pole, the ends passing through the arm and secured with nuts, on screwing the nuts well up it is found that the ribands cut for themselves places or grooves in the arm, and it thus becomes almost immovable.

The description of the manufacture has been general, without giving any particulars as to weight and dimensions; the following details, however, give particulars of some poles supplied to the Postal Telegraph Department for some special comparative experiments.

Riband Pole, total length 30 feet, 27 feet above ground.

	Cwt.	qrs.	lbs.
6 Ribands, $1 \times \frac{3}{16}$ ... ... 3 Angle Irons, $1\frac{1}{4} \times \frac{1}{4}$ ... ... 9 Flat Bars, $1 \times \frac{1}{4}$ (7 ft. 3 in. long), all wrought iron	2	2	25
Base, 3 Cast Legs	1	2	12
1 Plate	0	1	18
2 Blocks and Screws	0	0	17
	4	3	16

The above was supplied at a price including galvanizing at less than £5, or a little more than £1 per cwt., of which more than one half was of the best wrought iron.

The dimensions of a second pole 17 ft. 6 in. above ground, for curves or terminating are 20 ft. over all, or 17 ft. 6 in. above ground.

	Cwt.	qrs.	lbs.
8 Ribands, $1 \times \frac{1}{16}$ ... ... 2 Channels, $1\frac{3}{16} \times \frac{1}{4}$... ... ... 2 Angles, $1\frac{1}{4} \times \frac{1}{4}$ ... ... 4 Flat Bars, $1 \times \frac{1}{4}$, 5 ft. 3 in. ...	2	0	8
Base, cast cylinder	1	1	16
Base Plate	0	1	13
	3	3	19

including galvanizing, at a price equivalent to the former.

It will naturally be asked what are the advantages or disadvantages of such a pole, and what are the tests to which it had been applied. Taking the last first, it will be as well to give some tests to which an ordinary pole of this description has been subjected; but first it is necessary to combat an opinion that has been expressed by some persons as to the weakness of the pole in supporting a perpendicular weight. It has been urged that such a pole would shut up under a heavy weight, like a "Jack in the box;" but as facts are stronger than opinions, it may be stated that 2 riband posts, 10 feet by 8 inches cylindrical, supported on their tops a small platform, which was loaded to the weight of ten tons, without shewing any signs of sinkage, deflection, or collapse. It must be remembered that these same posts were made of 8 ribands, without any assistance from angle iron. They underwent this test for a lengthened period.

The following is an experiment to test the lateral strength of the pole:—

The pole was 20 feet in length over all, and was fixed longitudinally, so that the fulcrum corresponded with the ground line, the base was firmly secured and the weight was applied at the upper extremity of the pole.

No.	Weight.	Deflection.				
1	1 cwt.	$\frac{1}{4}$	inch	permanent	Set	Nil.
2	2 ,,	$\frac{1}{2}$	,,	,,	,,	,,
3	3 ,,	1	,,	,,	,,	$\frac{1}{16}$
4	4 ,,	$1\frac{5}{8}$	,,	,,	,,	$\frac{7}{16}$
5	5 ,,	$1\frac{7}{8}$	,,	,,	,,	$\frac{9}{16}$

the post bent close to the foundation plates, but when the weight was taken off the post came straight.

There are many experiments of a similar character which may be mentioned, but the above is considered sufficient, however, the experiments to be conducted on this form of pole as well as on other forms by the Postal Telegraph authorities, will, it is to be hoped, be communicated to the Society, as they will doubtless prove of the greatest value in demonstrating the comparative advantageous qualities of the several poles; with regard to the riband pole, the author would have been glad to have included the tests of this pole in the present paper.

The advantages claimed for this pole over wood are similar to those of any iron pole—freedom from the troublesome effects of "weather

contact" and greater durability. The latter is indubitable, the former unnecessary to describe before practical telegraphists.

Compared to iron posts generally, the advantages claimed for this pole are :—

Great strength compared with the lightness of the material employed.

Economy as to manufacture and material, its cost being less than the majority of iron poles.

The capabilities of manufacturing the pole to any required length or lengths, without being limited to 2 or 3 definite lengths.

It offers less resistance to the wind, from the small amount of surface exposed, in consequence of its open character.

Finally, its appearance, which is certainly more elegant and pleasing than that of any other pole; this advantage has been almost universally recognised.

Of whatever disadvantages it may possess, it will not do for the author to speak of that with which he is unacquainted; he leaves those points, therefore, to the meeting to express their opinion upon.

The Chairman said they had been favored with four interesting and valuable papers on the subject of Telegraph Poles, but the hour was too advanced to commence their discussion that evening; there was so much to be said that justice could not then be done to the subject. He thought it would be a great advantage, and the Council had ordered, that the papers should be printed and sent to the Members, in order that they might be better prepared to discuss this important subject at the next meeting. Some gentlemen present had had great experience in poles, both of wood and of iron, and he hoped they would look up all the information they could. Some would, no doubt, remember the experiments made by the Electric Telegraph Company, of placing wooden poles in iron sockets: he hoped some information would be given as to their durability and as to their strength. The discussion of the several papers would, therefore, be adjourned to the next Meeting.

The following Candidates were balloted for and declared duly elected:—

As Foreign Member:—

Professor D. Karl Zetzsche, Chemnitz, Saxony.

As Associates:—

A. S. Betts, Kurrachee.
H. Winfield Crace, 31, Lombard Street.
John Smythe, Valentia, Ireland.

The Meeting then adjourned.

The Thirteenth Ordinary General Meeting was held on Wednesday, February 26th, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

THE CHAIRMAN said that the discussion on the papers on Iron Telegraph Poles would then be commenced. Before entering upon the discussion, however, it was competent for the authors of any of the papers to make any remarks they wished. In the case of there being no such remarks, they would commence by reading a communication which had been received from Mr. Haynes upon the subject, and then the discussion would be commenced:—

Telegraph Department,
Bristol and Exeter Railway, Taunton,
24th February, 1873.

WOODEN POLES IN IRON SOCKETS.

The Electric Telegraph Company erected three miles of line, between the Yatton and Worle Stations, of the Bristol and Exeter Railway on this principle, as an experiment, about 20 years since, and I cannot learn that any renewals were necessary until last year. I then found that several of the poles had gone badly near the sockets, and others were rotted at the top. Those that were bad at the bottom were several inches below ground, and had evidently been in that position for a considerable time, they apparently had either forced the sockets deeper into the ground, or the sockets were buried too deep in the first instance. The defect in the top of the poles appeared to be caused by the bracket nails and arm bolts allowing the rain to penetrate more readily at those points than at any other. The poles did not look as if they had undergone any previous process before erection, those that remain are clear of the ground and comparatively sound, they are 18 feet in length, carry 10 wires, and have only recently been stayed; but as these poles are on a straight line, I am unable to offer any information as to their strength on curves. This

experiment shows that iron sockets preserves plain wooden poles about twice as long as when they are put in the ground in the ordinary way.

I am, however, of opinion, that when *well* creosoted, foreign timber is used for Telegraph poles they are as cheap, strong and durable as can be desired. There are seven miles of line on the Bristol and Exeter Railway (between Martock and Yeovil), where creosoted poles were put in during the spring of 1863, and these poles are as good now as when erected. The creosote during the hot summer months is drawn to their surface, and they look as black and glossy as if fresh taken from the tank. It is true, there is some difficulty in getting poles properly creosoted. There are a number of so called creosoted poles on this line that have only been standing a year or two, and they are already bleached on the south or sunny side and look but little better, and I presume, are only likely to last a little longer than plain poles.

The best kind of creosote and method of creosoting is, I think, worthy of some attention and consideration.

T. HAYNES.

Mr. Culley said he thought he might say that the poles of which Mr. Haynes spoke were not rounded at the bottom, although the rest of the pole was rounded. They had stood so long——

The Chairman: Can you state whether they had any preservative mixture of any kind applied to them?

Mr. Culley said he could not say that they had. There were other poles between Bath and Chippenham; these were fixed in screw sockets at the same time, and were quite sound at the tops. Unfortunately, the sockets cost twice as much as the poles.

The Chairman thought he ought to say that the question before the meeting was one simply of telegraph poles, not merely iron telegraph poles, and he should be glad to hear any remarks on any description of pole. Perhaps Captain Mallock would kindly state his experience of iron telegraph poles in India, as he had had a very large experience in that direction.

Captain Mallock said Major Webber's paper read at the last meeting, and also the Chairman's closing address, ended with an

inquiry for information as to the practical experience that had been gained of the use of iron sockets in the ground to carry wooden telegraph poles. He thought the history of the introduction of iron poles in India would be instructive on this point. Having no statistics within reach, the dates he should give might be slightly incorrect, but still they were near enough for the purpose required The first telegraph lines in India, in 1850, were erected by Sir W. O'Shaughnessy, on wooden poles. These being found to rot in the ground, the next step, about 1853, was to fit them into cast iron sockets, which were screw piles of Mitchell's pattern. These piles were about 3½ feet long below ground, and reached to the ground lines. It was then found that the wooden posts decayed at this ground line, and to remedy this the next step about 1855 was to supplement those screw piles with a short cast-iron socket which fitted into them, and reached about eight inches high up the wooden post. These also failed. Then in about 1856 were introduced Hamilton's standards, which, being apparently a sequence to the original cited, were 8 feet wooden poles fitting into galvanized iron tubes 8 feet high: these at first being fitted on to the original screw pile and afterwards on to a cylindrical socket, which had two cross-bars at the bottom. The galvanized tubes were at first filled with a cement of sand and resin to strengthen them, but, as this swelled with the heat and burst the tubes, it was discontinued. The next step was in 1860, when, still adhering to the first idea of a wooden pole in an iron case, the galvanized tube grew to 13 feet long, with four feet of wood at the top. Finally, in 1864, this last remaining four feet was thrown out, and thus the original wooden post finally became a complete iron one, which also for the first time was received from England in pieces eight feet long. These pieces pack the one inside the other for carriage, and are joined up on the line. Their education has commenced with their own experience, and indeed with the example of Siemens' iron posts which were used by Col. Stewart on the Persian lines about 1863. The experience of 12 years having made them adopt a complete iron post, that of the next six taught them that this iron post must not be of one fixed pattern, either in length or strength, but must be such a post that whilst retaining one general character throughout the line could be varied in its dimensions according to situations. They had not often the same difficulties of severe angles which

Major Webber spoke of with reference to English raised lines, but as they were not allowed to put their lines within 24 feet of the rails on railway lines, they were driven into very bad ground, and they had besides very difficult country. For instance, for about 30 miles below the Ghâts which were near Bombay, he did not believe that there were five miles of straight line, or that six consecutive posts could be found of the same length.

When they first introduced iron posts, they were all the one rigid length, so that they had to increase the length when wanted by adding wooden poles to the top. This by lengthening the average, without increasing the strength of the base was necessarily wrong; but having only one size of post, they had either to do this or to erect wooden posts. Latterly the required variations in height had been made by shunting specially made iron pieces on to the top or hollow of the ordinary posts, and using larger sockets for the larger pieces when put on at the bottom, and the variations in strength had been made by using poles of a larger size, called angle or terminal poles, but except that the ordinary pole would fit into the lower half of the terminal. There was no complete system for a generally graduated pole that would suit all requirements until last year. Their experience of about 300,000 poles told them what was wanted, and certain requirements were laid down which the manufacturer was left to fulfil. These were as follows, viz:— a complete post to be 40 feet long, in 8-feet sections, every two or more of these sections when fitted together to form, with the addition of a socket suitable to them in size, a complete post; all the sections to be of such a size that any one would fit into the next larger size for carriage. Calling the sections A, B, C, D, E, all of them were to be of one taper, this taper and the sizes to be such that the section E should fit sufficiently well below those of the original pattern; also that the strength of the whole or any three or four portions, when connected, should not be less than the strength of the two smallest. Thus, post B, C, D, E, when connected, must be the same strength as B and C, &c. These requirements had been met by a post of which the taper ran from 2¾ to 9 inches, or 1¼ inch in 8 feet. Theoretically, this was not the best form to get a gradually increased strength downwards; but, as the taper must be kept within bounds on account of fitting the brackets, the extra strength was obtained by increasing the thickness of the metal in the lower pieces; and in

testing the posts when they were accepted, the whole 40 feet post bent like a fishing rod in one curve.

It was not, however, to be understood that these wrought iron tubes were considered to be the best possible telegraph pole that could be made, but the history he had given of how iron poles were introduced would show how they came to adopt poles of a diameter nearly the same as that of a wooden post. In fact, if they took a 40-feet fir tree, and knowing the height or the strength they required, cut from it a certain suitable length, it would be nearly the size of the sections of the post that they should use. The objections to posts made of thin wrought iron were, however, that in certain localities this thin iron rusted away. Supposing thin posts were $\frac{3}{8}$ths thick, and $\frac{1}{12}$th rusted away, $\frac{7}{24}$ths of an inch remained; but supposing it be only $\frac{1}{8}$th thick and $\frac{1}{12}$th rusted away, only $\frac{1}{24}$th remained. He had before now taken posts that had been ten years in use, and brought in when the line was transferred from the road to the railway for new posts. On the other hand he had known some which were exposed to salt water rust away in four years. As far as strength for weight went the thinner the iron and the larger the diameter the stronger would be the post. This quality, however, did not counterbalance the objection of the thin metal rusting under certain influences. However, he considered it indispensable to have a post in a series such as they now had, and of which, as he said before, they could take any portions to give them what they wanted for particular places.

There was no reason for any other lines (when introducing a taper series) to start with such a large diameter and such thin metal; but if the metal be much thicker and the diameter less, then arose the next difficulty of shunting the joints together. The heaviest metal they had yet used for these taper wrought iron tubes was for some masts in Calcutta, of which the lowest sections were made of $\frac{3}{8}$ in. boiler plate.

The number or the strength of posts that we use per mile along railways and in open country is determined from the following equation—

Let w = total weight of wire per mile,

n = number of posts,

s = breaking strain of each post,

Then, $n = 4 \dfrac{w}{s}$.

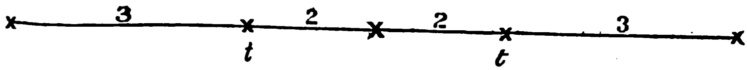
On account of wind contacts, n cannot be less than 17·6 per mile or 100 yards apart, and where W is small, s may be reduced by taking a smaller sized post.

The equation is not a calculated one, except that the proportion of $n = \frac{w}{s}$ is necessarily correct; the constant (or empirical) multiplier of 4 was obtained merely from experience of lines that were or were not blown down in cyclones.

They also adopted a very simple but absolutely correct method of fixing their stays in a position opposite the resultants of strains at angles, and also for setting the cross-stays at right angles to the line. The method is as follows:—

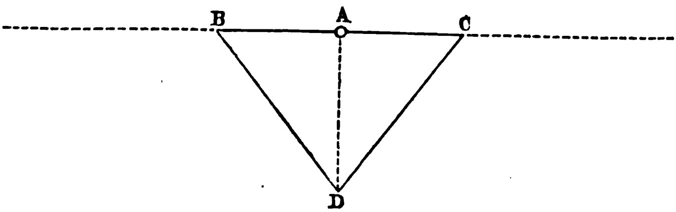
I.—For Cross-Stays.

Take a string 10 yards long, with knots in the positions shewn—



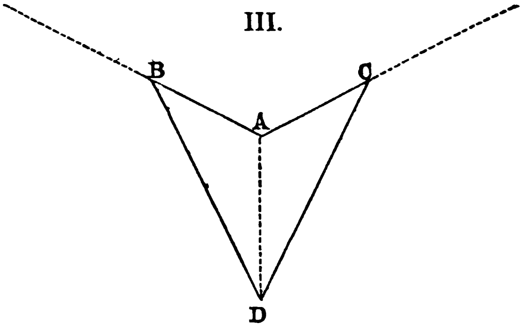
Let A be the post to which it is desired to fix the Cross-stays. Grasp the two knots (t t) in the hand, let your assistants hold the two end knots, and place them in the alignments at B C, they will then be three from you. Then taking the centre knot, pull it out to D; the angles C A D, B A D, will then be right angles, and the distance A D = 4 yards or 12 feet, which is the required distance for a cross-stay. Having found the point D, leaving the ends of the string at B C, cross over to the other side of the point, and you will have the points for the opposite stay.

II.



Similarly, letting A be the post at an angle, mark off B C and D with the same string. The triangles B A D — C A D will be similar, and A D be necessarily opposite the resultants of the strains; but as the line A D will not be 12 feet, it must be prolonged to the necessary length.

III.



Where they had uneven sizes of wires at angles, their system of procuring their wires by weight (all the weights being multiples of each other) instead of by B W G, was a material assistance to calculating the position of the resultant strain. For instance, supposing the posts on either side of the angle to be equidistant from it, all that is necessary is to describe a parallelogram with sides in proportion to the weights of the wires.

As to base plates there were many soils where they were not required, but he did not see the objection to them that Major Webber urged, viz: that they necessitated the removal of more soil than would be absolutely necessary. The base plates now in use varied in diameter from 22 to 26 inches, and were placed at depths varying from $3\frac{1}{2}$ to 6 feet; and as it was almost impossible either to dig a perfectly cylindrical hole 4 or 5 feet deep, or to get a line of posts up straight without making the holes long and narrow, the length being at right angles to the wire, the soil must be disturbed, so that the base plate might as well go in. In cotton soil large stones were generally found, which could be thrown on the plates and rammed down with the earth, making the new soil if anything firmer than that round it. Also, when on a long line, they generally tried not to put a strain on for about a month after the posts were up. The anchors to which the stays were attached were universally discs of cast iron. The stays themselves being of twisted iron wire. Struts they found were very seldom wanted. They had been made of two

pieces of T iron worked back to back; but he had constantly used two iron posts coupled together by means of wrought iron flat bar couplings.

With reference to Lieut. Jekyll's paper, on stays for two or three wires, they attached the stays to the bolts of the brackets; for more than this, above the highest pair of brackets was attached what they called a hooked clamp, which was a hook made of flat bar iron projecting about 4 deg. above and 2 deg. outside the highest insulator; a pair of clamps was then attached below the lowest bracket, with a loop of wire between the stay then attached to the loop.

THE CHAIRMAN asked what was the breaking strain when about 18ft. out of the ground.

Captain MALLOCK replied that the contract was that any one section of the post should bear the same strain as the whole put together.

THE CHAIRMAN: 500 is your breaking strain always?

Captain MALLOCK: Yes.

Mr. SIEMENS said that, as a paper of his was one on the list, he wished to premise by stating that it was not intended to be a paper, but simply a statement of facts regarding the construction of the posts which he introduced many years ago, and that statement was intended as being in addition to the information which was brought before the members by Major Webber in his paper. He had hoped that Major Webber would have brought before the meeting the construction of posts of that type, in order that they might have benefitted by the results of his investigation and inquiry into the subject; and he was somewhat disappointed to find that Major Webber confined himself to criticisms on the existing constructions and to a sort of *résumé* of what had been done. There were several parts of his paper from which he (Mr. Siemens) dissented, and he was quite sure that his friend, Major Webber, would be quite pleased to find that his propositions would lead to a discussion that would tend to the understanding of the facts of the case.

Twelve years ago his (Mr. Siemens') attention was first drawn to the necessity of iron posts for countries where wooden posts were subject to dry rot. He found that in all southern countries wood did not last above one-third or one-fourth the length of time that wooden posts lasted in this country—in fact, two or three years seemed to be the lifetime of a wooden post in South America or Africa. Though these posts might be specially prepared,

injected with sulphate of copper, or creosoted, such processes did not materially prolong their lives, and the necessity of providing a stronger material for the purpose became evident to him. The necessities of the case pointed to iron as the best material, and then arose the question, what is the best form to put iron into in order to support the lines of wires at a given height? In the first instance he turned his attention to the tripod. The tripod was a very strong form, a very stable form, and by means of that construction he succeeded in making a strong post. But this construction was not satisfactory, because each limb of the tripod was not sufficiently strong in itself to stand independently. Each member of the structure required support. These supports had necessarily to be lateral supports, and these lateral supports, while they added to the weight and to the expense, did not contribute to bear the strain that might be applied on the top of the posts. Therefore, he abandoned that form in favour of a tubular construction. Now the tube was evidently the right form for bearing a strain in all directions equally, because each part of the material was at the greatest possible distance from the centre; but a cylindrical tubular form was evidently a bad construction, because it would give an excess of strength near the point of suspension, and a minimum or relatively insufficient strength at the base. Therefore the conclusion he drew from this was that a conical tube would be the best. But, on going minutely into the question, a conical tube did not seem to fulfil these conditions, but a tube of a parabolic form externally was the form which gave the maximum of strength. The drawings on the wall showed the upper portion of such a post to be a wrought-iron tube of that parabolic form which, when a weight was horizontally applied at the top, gave equal deflections in all parts. The next question was, What is the proper thickness of the material of such a tube? Captain Mallock had just said that the larger the tube and the thinner the metal, the greater would be its strength. He dissented from that entirely. If more stiffness was gained by increasing the diameter at the expense of the metal, the strength was not increased in a tube such as that shown, which had a thickness of metal of about a quarter of an inch, and a diameter of about four inches. If reduced in thickness it would collapse, and collapse with a less weight than the actual tube would bear. If, on the other hand, the diameter was decreased and the thickness of metal increased, it would break or bend with

a less weight. Therefore, there was a point of proportion between diameter and thickness and proportion, which gave an absolute maximum of strength. He did not agree with Major Webber in saying that telegraph poles might be constructed in a hundred different ways to suit the requirements or the fancy of their designer. He looked upon the construction of a telegraph post as one of the most definite things which an engineer could have put before him. Whether the pole was to support straight wires, with only moderate lateral strains to bear, or whether it was to be an angle post to resist the strains of wires pulling in opposite directions, the problem always was to support a strain at a certain height above the ground. This was the one of the points which could be solved in a thoroughly mechanical manner. He had found that the lower portion of the post, which was exposed not only to the strain of the wire but to the moisture of the ground, ought to be of a different material to wrought-iron which corroded very readily, and, therefore, took cast-iron, which seemed to be the most suitable material. Then came the next point. How is this base to be fixed in the ground? The natural suggestion was: make the whole tube uniform, and put the post in and ram it all round. But what was got then? A very small post indeed as compared with a wooden post—small in diameter, necessarily so because limited in weight and constructed of a material of greater density. It would therefore, be necessary to put the iron post deeper in the ground than the wooden post. Major Webber claimed for the posts constructed on his principle two advantages over those proposed and used by Mr. Siemens, (1) less excavation, and (2) saving of material. He thought that in both instances Major Webber was mistaken. Firstly, as regarded excavation, Captain Mallock had already stated that if a hole could be made 6 feet deep, it had to be made of such a size that a man could get in. Now the base plate for supporting his (Mr. Siemens's) post was less than 3 feet in diameter, 2 feet 8 inches, or thereabouts, so that in reality no larger excavation would be required for the pole with the base plate than for the pole without a base plate, the only difference being that for the former the excavation need only be 2 feet 6 inches, or 3 feet deep, while for the latter it would have to be 6 feet deep. With regard to the weight of metal, Major Webber had said that the weight of the base plate might be saved by extending the length of the tube into the ground. The depth he gave—it was a very ordinary depth—was 6 feet. Now the extra

length of the cast iron tube—the difference between 2 feet 8 inches and 6 feet, *i. e.*, 3 feet 4 inches—would weigh 89½ lbs. or 90 lbs. His (Mr. Siemens') base plate, which was a dished wrought iron plate, weighed 30 lbs. Therefore a saving of 60 lbs. in weight was effected. But that was not all. Major Webber had said that he (Mr. Siemens) derived the strength of his post from the base plate, but that he (Major Webber) derived his strength 2 feet from the bottom. Therefore Major Webber's post, the same absolute height out of the ground, of the same absolute length between the turning-point of his level of 18 inches, to be of really the same height out of the ground, ought to be 18 inches longer than his (Mr. Siemens). Not only did he (Mr. Siemens) save 54 lbs. in absolute weight (supposing he made his tube of the same strength as Major Webber's), but he was enabled to raise the strength of his post as if he had it 18 inches longer. But the base-plate had another advantage. If it were put into the ground, and the earth filled up over it the post was absolutely fixed. The strain might come in sufficient severity to move the base-plate, but the moment the strain left it the earth fastens it down. A mere iron or wooden post put into the ground, if once shaken, would always be loose, and the iron post had a very great disadvantage as compared with the wooden post, because it had less surface, and that surface was so smooth that it slipped through the earth much more readily than wood. The advantage of the base plate was that the weight of earth itself fastened the post. With regard to the amount of earth, that, he thought, according to Major Webber's opinion, seemed to be much lighter than was absolutely requisite. He had never found an iron post put 2 feet 8 inches in the ground to be torn up. The line in which the earth would pass, if the strain were applied, would be such as to render the earth on the base plate not only a dead weight, but the earth would be lifted away at an angle of about 45 degrees, and the frictional resistance to moving that earth would come in aid of that dead weight. It was quite remarkable how firm posts of this description stood in the ground. In fact, it was only imitating nature. If a tree was uprooted, it would be found that it took its strength near the surface of the ground. The roots spread at once, and if the enormous pressure brought to bear against a large tree standing alone in a field with a gale blowing against it were calculated, it would be surprising to account for its holding its ground against a pressure

of perhaps from 40 tons to 100 tons acting against it. He, therefore, strongly maintained the base plate to be a most important feature in the construction of a telegraph post. Again, the base plate enabled the load to be much more equally divided. If a socket were carried down to the 6ft. limit into the ground, it would be necessary to carry it, in order to be safely out of the ground, 4ft. longer, and this would necessitate a cast iron tube of at least 10ft. and perhaps 12ft. in length. This would be too ponderous a piece of metal to carry into countries where transport was difficult. The cast iron base was by far the heaviest portion of the post he proposed, and by carrying the post in three parts of convenient weight and size, mules and other country transports were quite equal to the work. Captain Mallock had described the method which he had used in India of making short into long posts, and he mentioned it as an advantage in favour of the particular construction which he had adopted in India. It was certainly an advantage to be able to increase the height of a post when necessary; but it should be remembered that the base was the most important part of the post, and if the base were carried to where the post was required, this could be more satisfactorily effected, for, unless it was known beforehand whether the post was to be a high one or a low one, it would be necessary to carry the strongest base everywhere. The way he should generally manage was this: There was one strength of post to support the line where it was straight, and another strength of post for corners, known as "stretching-posts." These corner-posts bore half a ton generally of horizontal strain brought to bear upon them, and the others generally about 5 cwt. But if a higher post were required to cross the road or otherwise, a stronger post was taken and a lighter tube put upon it by being dropped simply upon it. By that means much higher posts were obtained and the strains throughout were proportioned to their strength. In that way he accomplished what Captain Mallock gained in the way he described. He would still draw attention to the fact that, although a great many varieties of construction were talked about in reference to a telegraph pole, yet in reality there could only be one construction in principle, whatever were the variations in detail. There could be only one construction to give the maximum of strength for a given height. Then there were other considerations which ought to be attended to. The post ought to be light, and ought to resist oxidation

for the greatest possible length of time. This was a desideratum which ought to be met, and the conditions on which it ought to be met were not manifold. They would find that they would come very much to a definite mode of operation or construction in applying all these different requirements. He did not know that he had any further remarks to make upon the subject.

Colonel Robinson, R.E. (Director-General of Telegraphs in India, responding to the Chairman's invitation), said: Not having been present at the last meeting, nor at the earlier portion of the present one, he had lost much valuable matter, no doubt. Captain Mallock had informed them how we commenced the use of small iron sockets, which, in course of time, were increased in size from 6 inches above the ground to 6 feet above. Mr. Siemens had described the advantages of his pole, which was found to be an exceedingly good one at short distances from the sea, although nothing escaped the action of the sea air. Another description of pole was also used, the Hamilton pole, which gave the maximum strength for a given quantity of wrought iron of any pole he was acquainted with. According to theory, the larger the diameter of the tube the greater the strength, and if they had the tube too thin it was speedily impaired by oxidation. Galvanizing did a great deal to preserve the iron poles, and when so treated they would perhaps last for twenty years. Some of the finer woods of Ceylon last a long time, particularly sandal-wood. In blue mud soil wooden poles stand better than iron. Captain Mallock, during the two or three years he had been in England, had been engaged in making improvements in the Hamilton pole, and the manufacturers attached a plate to the bottom by a screw, which had the same effect as in the Siemens' pole, and he (Colonel Robinson) believed there was great advantage in it. The cast-iron sockets in which the wrought-iron tubes rest, used to terminate at ground level, they are now 15 or 16 inches above it, and have been made lighter. They diminish in thickness as they come from the place of insertion into the ground; weight 1½ cwt., and the compound pole capable of bearing 600 cwt. of dead strain perpendicular to its axis. He had not found any other poles, with the same quantity of metal, at the same price, and of the same weight equally strong, although he had seen in India another kind of pole used on railways, which partook a great deal of the character of signal posts on railways, but at a price so enormous that they could never use it for the telegraph. He believed

they had been made to utilize a quantity of old scrap and angle iron. He understood that description of pole would bear two tons of strain.

THE CHAIRMAN: Is it a compound built pole?

Colonel ROBINSON: Yes; compound built, 18 inches diameter at the base and 4 or 5 inches at the top of a square section. Major Webber had remarked upon applying struts and stays to the iron posts. He (Col. Robinson) had often thought whether with poles like those the strut might not be carried across the road and strutted on the other side of the road; by that means they would get a stay which would take off the strain almost entirely, and save the pole.

THE CHAIRMAN said Colonel Robinson spoke of a pole which he regarded as stronger and better than any other he had seen. He thought the meeting would like to hear more about that. Colonel Robinson spoke of a tapering base.

Colonel ROBINSON said the Hamilton post had a cast-iron socket 4 ft. 6 in. long, of which about 3 ft. 6 in. went into the ground, and tapering to 3 or 4 inches at the bottom. A screw was cast into the lower portion of the socket, and on to that was secured the base plate. It was supposed to be worked with little difficulty in making the base plate to screw on to the cast-iron socket, everything else being sufficiently accurate. The base plate screwed on was fixed with nuts and bolts on to the bottom of the socket.

THE CHAIRMAN asked what was the form of the base plate?

Colonel ROBINSON: Cast-iron, of corrugated form.

Captain MALLOCK stated they varied from 22 inches No. 2, to 28 inches No. 5—the length of No. 2 being 4 feet, and of No. 5, 8 feet.

Mr. SIEMENS, in reference to Capt. Mallock's illustration, asked why he made the lower tube thickest near the top and thinnest near the bottom. There could be no doubt he said, about one thing, viz: that the maximum strain was immediately over the socket plate, and it was there he had always endeavoured to get the greatest strength. If Capt. Mallock adopted the buckle plate he (Mr. Siemens) did not see why he should have this screw. It might be matter of opinion, but it appeared to him the screw was less solid than the bolting, but what he asked was why that was not made the strongest part of the post?

Col. ROBINSON replied that the cast iron socket was stronger than was required. He had never known an instance of a socket breaking. It was in fact the strongest part of the post, quite as strong as wrought

iron. The taper downwards was to make it lighter. The wrought iron tubes above fitted over to prevent the water getting into the socket.

Capt. Mallock, after giving some further illustration of this arrangement, stated that the screwing on of these discs was a thing they never expected to get at. They were trying to get the base plates put on in any possible way. The first thing was, they began with two cross bars at the bottom of the socket, and to do that they clearly wanted base plates, and they were coupled with four lugs. That gave trouble, and then the manufacturer said he would try to have them screwed on, in which they ultimately succeeded.

Mr. Ayrton said one thing occurred to him, Captain Mallock mentioned that one of the last stages of the Indian posts was an almost entire iron post with a wooden top. To prevent cross leakage iron wire was twisted round over the brackets, and fastened to the iron post at the top by a small screw. This however was totally inefficient, and did not prevent cross leakage. The resistance of the post was apparently greater than that of the insulators and iron brackets. He would suggest, in the case of iron posts with wooden tops, it would be well to carry the leakage wire into the ground, and not attempt to fasten it to the posts. An attempt was made to solder the wire to the post, but that failed. That was the reason for giving up the last piece of wooden top which they adopted, and having entire iron posts.

Mr. R. von Fischer Treuenfeld: Having erected many thousands of iron telegraph poles, and directed the maintenance of telegraph lines having such poles, he begged to draw the attention of the gentlemen present to certain points which experience had proved to him to be of great importance concerning the best construction of an iron telegraph pole. The engineering rules with regard to the true principles of construction of the pole had been most ably laid down in the papers read, and he had nothing to add. His remarks were of a purely practical nature, but such as they were he believed they should not be omitted when judging the merits of a pole. Iron telegraph poles were generally tested by applying a weight to the end of the pole and observing the bend and break. The maximum breaking strain of the pole compared with its weight and price was often erroneously considered as showing the best pole. By adopting this course experience soon proved that they had been greatly mistaken in their judgment, as the same breaking tests, when applied

after the pole had been in the ground for a number of years, gave quite different results from those obtained when the pole was new. Therefore poles ought to be judged after a number of years of service, not when coming out of the manufacturer's works. This observation led directly to the question of the choice of metal for the pole, whether cast iron, wrought iron, or the two combined would give the greatest durability. The destructible influence of poles were oxidation in the open air, or corrosion of the part underground in consequence of chemical action between the iron and the ground. It was evident that iron poles with a smooth surface, small diameter, but strong metal without rivets or joints would be less liable to oxidation than those where moisture could enter into a number of joints and openings. From this point of view the smooth round shaped pole would be preferable, and it ought to be borne in mind that oxidation is a very dangerous enemy in hot climates. Galvanizing was often asserted to be an infallible remedy against oxidation; but unfortunately it was not so, and proved after a time to be no preservation at all, at least in hot and damp countries. Galvanizing had besides the peculiar and fatal quality of weakening the tensile strength of the thin wrought iron, and therefore, was more a disadvantage than otherwise to an iron telegraph pole. The part of the pole underground was much more liable to corrosion and consequent destruction than that portion in the open air, and there were places where the soil contained such an amount of saltpetre, or other salts destructive to iron, that thin wrought iron poles would not stand a fortnight with safety in some grounds. It is known that cast-iron is far less susceptible to corrosion, and, therefore, cast-iron would be the preferable metal for iron telegraph poles; at any rate, for that part of the pole which comes in connection with the ground. Wrought iron, being the stronger and more flexible metal, offered greater advantages for the upper part of a pole. Another important agent of destruction of telegraph poles was the wind, and it was evident that poles, which offered the least possible surface to the wind, and which avoiding angle-shaped surfaces, would suffer the least from gales. This consideration urged the adoption of small diameters for poles.

He did not quite agree with the views expressed in the first paper read by Major Webber as regarded the utility of ground plates; and he did not see the disadvantage of a large excavation for the iron

plate, when compared with poles without plates. Supposing they had a pole with the same plate 2ft. 6in. square as mentioned in Major Webber's paper, and compare it with his pole without plate. The latter would have to be interred 6 feet, and he doubted that any one would dig a hole of that depth without making it at least 2 feet 6 inches across. Therefore, there would be no gain in that direction, as the former required a hole of the same width, and only interred half the depth. He thought that the very question of excavation stands in favour of the adoption of ground plates, especially if the line had to be erected on rocky grounds. In summing up his observations, he begged to say that iron poles could only be fairly judged by tests after several years standing in the ground; that oxydation, corrosion, and storms, greatly influenced the durability of poles, and that, to obtain the best durability, the following principles of construction ought to be adopted:—

1. Cast-iron lower part of pole.
2. Wrought iron upper part.
3. Small diameter of upper part.
4. Strong metal.
5. Smooth surface.

Comparing these views with Mr. Siemens' pole, experience taught him that that construction comes as near to a perfect telegraph pole as could be.

Mr. Graves said, with reference to the main question, viz., that of iron *versus* wood, he was afraid he was sufficiently purblind to see it only through one medium, and that of comparative cost; and so far as any general deduction he could gather, iron poles were a very great deal more costly than wood, and he wished to say as little as possible upon the merits of articles which were far too dear to be used generally.

Mr. Culley thought perhaps the meeting might wish to know from him why he had not used iron poles largely, and why he had not adopted the Siemens' pole, which was believed by many to be the best mechanical construction, giving the greatest strength with smallest cost. Iron poles, he believed he was right in saying, cost about three times as much as wood; he, therefore, looked at them through the same spectacles as Mr. Graves. He used wood simply because creosoted wood did last a very long time indeed, and cost about one-third what iron would. The only case in which they used iron

poles was where they were compelled, by opposition to wooden poles, to do so; that was to say where persons living in certain localities objected to the telegraph unless they carried it through on iron poles; and he had found this—that if they were to plane up a pole to a beautiful and perfectly true taper, and told people it was a wooden pole, they would not like it, but if they told them it was an iron pole they would admire it. He thought the public was very much led away with reference to poles, as it was with respect to quality and ornaments. An ornament might be as beautiful as could be designed, it might be perfect in form, but if it did not cost a great deal of money it was thought little of. He believed people liked iron poles because they cost more than wood; not because they were really more sightly. He must, however, confess that the pole before him (Mr. Robert Lee's) suited the popular taste better than anything they could make in wood, and in a garden or conservatory would be very elegant.

In fact, the cost was so large that they never used iron except where they were compelled to do so by persons who would not allow them to use wood; and when they used iron, they were obliged to put up something that pleased the public; therefore Major Webber had had from time to time to invent a pole, and had to show the drawings to the people with whom he had to negociate for the construction of the telegraph. He had to pass through a certain town: he showed several drawings of poles, and they said, "We will allow you to pass through if you use this kind of pole, but will not allow you to do so if you use that;" and therefore the best pole was sometimes not adopted—not the pole which was best and cheapest in an engineering point of view, but the pole which happened to please the people of the town the telegraph was to be carried through. He might state that they could not have carried out the work they had completed if they had had to pay three times the cost for poles that they had paid. It was his hope that creosoted poles would last for 20 years, and he thought 20 years was quite far enough to look ahead.

The Chairman asked Mr. Culley if he could give any information as to the results of Boucherising; also as to the durability of those wooden poles which were put into iron sockets.

Mr. Culley replied that the poles in iron sockets had lasted very well; they had lasted perhaps twice as long as they would have done had they been set in ground, where the socket was well above the

ground, and where the pole fitted well into the socket, so that the water did not get in; but if the water got in, the pole rotted very quickly. Creosoting appeared to preserve the poles for a long time—in fact, no poles so treated had yet rotted, but it was only fair to say that the experience with them had not extended over many years. He believed, however, there were some creosoted poles 14 or 15 years old. He had had some experience with Boucherised poles——

THE CHAIRMAN: Sulphate of copper injection?

Mr. CULLEY said sulphate of copper injection answered well in the majority of instances; but there were some situations in which poles so treated rotted as quickly as those which were unprepared, and some had lasted even less time than other unprepared poles set up alongside them; but that had happened in this way—the unprepared poles were of hard full grown timber, well dried before they were planted. Hard, well-grown timber would not take up the copper, so that in applying that process they were obliged to use timber of open grain, and the worst they could get. Scotch fir was the best, and naturally, if the impregnation of the pole was imperfect, as would sometimes happen, the timber being bad would last much less time than unprepared timber of good quality. He found that Boucherised timber did not last in very open sand or in peat soil. He thought generally, he might say, the copper process answered very well, though not so well as creosoting. The cost of each process was about the same.

Mr. GRAVES would mention a fact within his own knowledge bearing on the remarks just made. In 1870, it was necessary, for the Post Office purposes, to erect additional wires between Nottingham and Grantham, when it was found that the wooden poles were all sound, though, to his own knowledge, the creosoting was not well done. In 1862-63, he was concerned in preparing between 6,000 and 7,000 poles, and the drawback of that system in this country was the difficulty of getting timber suitable for the purpose. They looked for open-grained timber to receive the preparation. The timber selected, though open-grained, had become closed by time, and the result was, in many cases, though the poles were kept under pressure for three or four weeks, they were imperfectly injected, and the result was but little extra duration of the material. Between Huntingdon and Peterborough, where the soil was partly peat, some poles which were Boucherised nearly five years before they were erected, showed symptoms of dry

rot to so considerable an extent that, on the occasion of a number having to be shifted, it was found necessary to remove them.

Mr. Culley wished to add that Boucherising did not answer, unless it could be performed in the locality from which the poles were cut, so that they could be submitted to the process within a few hours after the timber was felled.

Major Webber (in reply upon the discussion) said: perhaps the title he gave his paper was a somewhat ambiguous one, viz: the Application of Iron to Telegraph Poles. When he first came to write a few remarks on the application of iron, he was not aware that Mr. Culley was going to investigate the subject as to the means in which iron could be supplied; he, therefore, at that time proposed to have enlarged on the subject, instead of giving a mere *résumé*, as Mr. Siemens stated he had done, in the short paper he read at the previous meeting. When he found that this subject had become of such moment that the Post Office was going to investigate the question of the relative strength and value of iron poles, and the application of iron to telegraph poles, to a great extent, his mouth was shut as to any opinion he might give as to the pole of the future to be used in this country. Perhaps it would be wise, at that period of the evening, to go at once to the point on which Mr. Siemens found he could not agree with him—that was on the expediency of the principle of the iron poles being terminated at the lower extremity with a base plate. The existence of this plate in a large number of places, as used in various countries, was a fact, as proved this evening by Colonel Robinson and Capt. Mallock. Facts were stubborn things; and he found in Mr. Siemens' paper there were 180,000 facts—that was 180,000 cases which he described in which a base plate had been applied. Now, perhaps he was very bold in saying more on this subject, because, as they observed, Mr. Culley had stated he considered Mr. Siemens' pole was the best iron pole in point of mechanical construction. Mr. Siemens had not told them the probable weight which that base plate carried. [Mr. Siemens: Half-a-ton] He made it a great deal more than that. If they took the weight which this pole carried as represented by a cube, of which the side of the square was equal to 2 ft. 8 in., they got a mass of about 1 cwt. of earth; but he (Major Webber) said this pole carried or was kept down by the mass which was equal in area to double the size, or four times the size, of the area of the plate, which would give them

an increased mass. The effect of the upper pressure of the pole extended to a distance of 2 ft. 6 in. on each side of the pole, and, including an area of ground of the space of 5 feet square, gave a weight of 1½ ton of earth, if that earth was piled up round the pole as shown in the drawing. But there was one point missed in his paper. He had said he doubted the expediency of applying a base plate to a pole which had to deal with such forces as he described in his paper. Mr. Siemens, in describing a telegraphic pole, had drawn a comparison between it and a tree. If they examined the trunk of different kind of trees they would find that the tree which fell easiest was that which had no depth of earth, but the roots spread out and covered an area of ground of limited depth beneath the surface; whereas the oak was a tree the roots of which penetrated directly into the ground beneath, probably in the case of the oak to a depth equal to the height of the tree above the ground; but he thought, although the tree was in his favour in that respect, he would concede the tree was a good comparison in the way in which Mr. Siemens used it; but then the tree was not a telegraph pole, and had not to do the work which the pole had to do, for instance, all the tree had to do was to resist the shocks it received from the wind. In doing so, it was in the case of a tree with the roots spread out a short distance in the ground, that tree when attacked by the full force of the wind shook in its top, and to a certain extent loosened the roots, and in time the ground consolidated over the roots, and the tree was as firm as ever after the storm; but in considering the circumstances of a telegraph pole they must regard it as subjected to a constant pressure in one direction which a tree could not be said to have. Now, of course, if that difference of circumstances was admitted then they had only to analyze those forces, as he did partly, and they found it was not in that way they kept a pole steady which had to resist a constant strain in any one given direction. Mr. Siemens had said the pole in his case saved in weight below the ground. Now it was entirely a question of where the point of fixture should be. In this pole the point of fixture was evidently on the base plate. In the pole he (Major Webber) had described the point of fixture was close to the surface, and if that were so, it was evident that was the strongest point, and any metal beyond that was wasted, inasmuch as it was more than was required to bear the strain it had to carry, when that was the point where the strain was resisted.

Mr. SIEMENS: You will have to put armour plate over the other.

Major WEBBER: Mr. Siemens, in advocating a base plate as against the pole without one, mentioned the iron pole being smaller than the wooden pole, the smaller surface force against the ground; but he (Major Webber) thought any one who had to fix a pole to resist the strain he had described, would never think of attempting to leave the pole in such a position, as that the security of the pole alone was the surface brought to bear against the ground, but they would put in a large rugged surface, in the shape of a stone, against which this would rest, to which would be communicated the whole pressure. With regard to the amount of earth which was to be taken out, comparing the smaller hole with a pole going 6 feet into the earth, it was true the amount of earth they took out here would be 17 or 18 cubic feet; but he maintained that this ton and a half which this pole was supposed to be influenced by, would not be able to keep the pole perpendicular when subjected to the horizontal strains he had referred to; whereas, if they put a pole 6 feet into the ground, they could, by ordinary appliances, make the ground hold a pole in a way to resist those strains, if the pole was only strong enough to bear them; but, the fact was, he thought a cylindrical hole could be made 6 feet deep, without taking out more than the 16 or 17 feet of earth which was required to get this base plate in. He thought he had said sufficient to show that, while admitting Mr. Siemens's 180,000 stubborn facts, he had made out a case in favour of poles which had to do the work which he described in his paper.

Lieut. JEKYLL would ask Mr. Siemens one question with reference to the strength of the junction at the base plate. How was the pole to fail if pulled up by great force? Would it raise the whole of the earth, or tear at the point of junction?

Mr. SIEMENS believed he had succeeded in making this pole equally strong in all respects. The strain applied at the top would perhaps bend the whole pole over to such a point as to approach the ultimate strength; then it would be matter of accident whether it broke in the cast iron. Most likely it would break near the ground line in the cast iron. There was an advantage he believed in the base plate being flexible—it saved the pole giving way in this joint. It would begin to yield a little, and if the ground was not firm possibly it might lift the earth up; but that he considered was the most likely point where the pole would give way. It would be useless to make it firmer in the earth than it was now.

Colonel Robinson, R.E., said, with regard to the adoption of iron in preference to wooden poles, it was forced upon him, not as a matter of economy, but of necessity. In temperate and northern climates wood lasted so well that he was glad to use it instead of iron. The cost of iron posts was very serious, being three times that of wood, and if the prices of iron and coal went on increasing as they had done, iron poles would become quite prohibited. On some of the mountain lines, wooden poles, which had been erected eight or nine years ago, were as good as when first put up, whilst the poles of the same kind of wood in the plains of India had not lasted a year, owing to the attacks of the white ant and the dry heat. In soft marshy soil, wood lasted very well: the damp did not affect it in the same way as the dry heat, whilst iron was utterly useless. Wood had been used wherever it was possible to do so. He did not think iron would be advantageous in England if the prime cost was three times that of wood.

The Chairman said they had four very interesting papers, and a very interesting discussion. They had had a very valuable paper from Major Webber, and he (the chairman) regretted with Mr. Siemens, and others, that he did not favour them with his views as to the form of pole which he would have been led to adopt, if it were left in his hands; Lieut. Jekyll had given them valuable diagrams and formulæ, which would be of great use in the office in calculating the strains on poles. He wished they had been made rather more practical by giving the actual strains which exist when wires of different gauges were strained to their ordinary point, so that instead of having always to calculate, the workmen might have a table to carry with them to inform them what the strain was with a given gauge of wire and a given number of wires; but they would hardly be able to calculate from Lieut. Jekyll's formulæ. Mr. Siemens had given them a valuable paper, and described fully a pole which had the obvious advantage of being constructed in two or three parts, and he (the Chairman) thought in the combination of wrought-iron and cast-iron the best materials were employed, and in their best position. There had been an interesting discussion on the use of base plates, as opposed to the use of poles without a base plate. He must say he thought that where base plates were employed, it was necessary to have great strength of form and substance at the lower part of the pole, to resist the strain caused by leverage; and he agreed with

Major Webber in thinking the best form of pole was that in which the lateral strain was taken near the surface of the earth. No one had alluded to the advantage of making a pole stronger in one direction than another. On all telegraph poles the strains were greater on the sides than lengthways. Lengthways they supported each other by means of the wires, but sideways they had the strains of wind, and the strains caused by curves and deviations in the line; and, therefore, he wondered no pole had been brought forward made stronger in one direction than another. His own idea was that the best form of pole was the oval, or which had ribs down the side to give greater strength laterally than in the longitudinal direction of the line.

Mr. Lee had shown them a very elegant pole indeed, but had not made much observation upon it. He imagined from what he gathered, Mr. Lee scarcely intended to put it in competition with the pole of Mr. Hamilton or that of Mr. Siemens, but that it was rather intended for special cases. Its strength, compared with weight and cost, would compare favourably with ordinary poles, but he thought it would not be durable, on account of the large surface exposed to oxidation. With regard to his own views as to the best form of pole, he thought the best form was one very much like the Siemens' pole, consisting of a long cast-iron base and wrought-iron tapered top, strengthened with a rib on each side, and one or two fins to resist lateral pressure. The cast-iron base below the ground to be small at the bottom and larger at the top, to give lateral strength. He also thought the use of the screw in screwing in such a pole was decidedly an advantage. He thought there was no necessity for digging a large hole and taking up the soil, but that such poles might be screwed very firmly, removing only sufficient soil from the top to place the fin plate in its place. He could have wished more information had been given as to the result of the use of screw socket poles and other kinds; but at the same time they must congratulate themselves upon having had a very interesting discussion, and it was now his very pleasing duty to propose a vote of thanks to the authors of the several papers.

The Chairman announced in conclusion that this was the Anniversary Meeting of the Society. They held their first meeting on the 20th of February last year, and he was glad to be able to congratulate them on the very satisfactory progress the Society had made. They were, in point of funds, very well off; and, in respect of numbers,

their progress had been most satisfactory. This time last year they had 110 members of all classes—at the present time they had 409.

The following Candidates were balloted for and declared duly elected:—

As Members:—

Colonel Douglas.
John P. Hooper, The Hut, Mitcham.
Professor Clerk Maxwell, F.R.S., Cambridge.

As Associates:—

J. J. Allen, India Government Telegraphs, Calcutta.
R. Boteler, India Government Telegraphs, Calcutta.
Thomas Bazeley, Post Office Telegraphs, Cardiff.
W. Belchamber, Post Office Telegraphs, Cardiff.
J. A. Bulmer, Post Office Telegraphs, Hull.
P. W. Cross, Post Office Telegraphs, Cardiff.
Thomas Elliott, Post Office Telegraphs, Basingstoke.
J. Haynes, Post Office Telegraphs, Gloucester.
F. Isherwood, Post Office Telegraphs, Cannon Street.
William Johnson, Post Office Telegraphs, Leeds.
J. C. D. Jones, British Indian Extension Telegraphs, Singapore.
Sofus Kaarsberg, 7, Great Winchester Street Buildings.
F. R. Lucas, Greenwich.
Charles Naser.
A. Oakshot, Post Office Telegraphs, Southampton.
A. Parmiter, Post Office Telegraphs, Reading.
F. Shepherd, Post Office Telegraphs, Brighton.
C. Taplin, Post Office Telegraphs, Cardiff.
G. Trenan, Post Office Telegraphs, Leeds.
S. Vyle, Post Office Telegraphs, Glasgow.

As Students:—

Samuel Hooper, Tremerton House, Clapham Park.
H. G. Cheesman, Post Office Telegraphs, Hull.

The Meeting then adjourned.

The Fourteenth Ordinary General Meeting was held on Wednesday, the 12th of March, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

The following communications to Mr. R. S. Culley, from Mr. Stout, Postmaster at Lerwick, were read by the Secretary:—

"POST OFFICE, LERWICK,
"11*th February*, 1873.

"R. S. CULLEY, Esq.

"WE are very much troubled here with "earth currents," or accidental currents of some sort, but I have neither heard nor read of currents of such intensity as those we experienced in the autumn and winter of 1870 especially. They would work relay of Morse beautifully, and I have often answered Kirkwall's call (as I supposed), a string of dashes, when, on taking out short circuit plug of galvanometer, I would find it was only earth, or accidental, currents, the needle vibrating right and left like a pendulum, sometimes regularly and then jerky. When currents came on strong we would have to drop working, it being impossible to get through. Even with single needles it is at times difficult to work, the needles being deflected right across the dials sometimes, and when receiving messages, I have had to keep the knobs of dial between my fingers, and keep turning it round to follow needle, before I could make anything of the signals. These currents seem to be strongest when there is considerable "aurora," of which we often have brilliant displays. I fancy it would be interesting to try and trace the connection between these, whether effect is due to distances, intensity, or direction of the waves of "aurora," or all combined. With brilliant "aurora," we often have strong currents, but on the other hand, sometimes we have very brilliant "aurora" and very little current, or none at all. These currents do not always pass from earth to earth (say from Lerwick to Kirkwall), as I have seen currents here so strong as to prevent working, yet Kirkwall had no trace of them, and this sometimes led to complaints, from either side, of inattention to calls, 72 Daniell's cells applied at Kirkwall having no effect on our relay. I have observed, also, on a shower of snow coming on, the currents commence with

G 2

shower, increase in intensity until shower was thickest; and then gradually decrease as shower lessened, going off entirely when shower ceased; this was more marked when the snow came in large fleecy flakes. *Hail* did not seem to have so much effect.

"ROBT. STOUT,
"*Postmaster*."

"POST OFFICE, LERWICK,
"R. S. CULLEY, Esq. "25*th February*, 1873.

"There is considerable difference in the appearance of the Auroras; the strongest currents, and those reversing quickest, I have seen, being when there was brisk motion, the aurora flashing along or across the sky; and in cases when, as it rises, the aurora appears as an arch of comparatively steady light there is generally little or no current, or then a current remaining steady for some time. Last night, for instance, there was some current on lines, needles deflected left the same as when I press left key, putting copper to earth and zinc to line, and on coming out of office I observed a beautiful auroral arch, extending from about N.E. to N.W. across the northern horizon. The needles remained to left for more than an hour at a time. The strongest currents I have observed were last week on our Unst section; the needles on this, or south half of line, were deflected left, the needle at Ulsta station (near the centre) was not affected, whilst the needles, or some of them, north of Ulsta were deflected right, the effect being the same as if copper had been put to centre of line, the current splitting and flowing *to* each earth.

"With reference to snow storms, we have had very little snow for several years; last year very little, but 70–71 one or two snow storms and it was then I noticed the currents, I think they were both + and —, but generally +. I observed it on galvanoscope of Morse (single current Morse), and needle was deflected to *left*, *i. e.*, in same direction as it was when Kirkwall put copper to line and line to earth; of course, when *we* put copper to line the deflection was to the *right*, the galvanometer in circuit between batteries and line; I had no needle instrument at that time.

"ROBERT STOUT."

"POST OFFICE, LERWICK,
"1st *March*, 1873.

"R. S. CULLEY, Esq.

"The appearance of the aurora varies very much, sometimes a steady arch composed of short vertical streamers; at others, steady arch, but streamers in a horizontal direction, and in both cases currents are generally very little, or if strong, steady, and more often give left deflection than right, *i.e.*, current passes to our earth. Again it assumes the form of luminous haze of pale greenish tint, and of varying density, floating or flashing about with great rapidity, and again of blood red color; the red aurora is not of such frequent occurrence as the other, and generally it overspreads the whole or most part of the sky, seeming to culminate, sometimes in a large ring, sometimes in a point in the zenith. The strongest current I have seen was on the 25th September, 1870, at 4 a.m. Our mail steamer arriving here about that hour, I had to get up and take delivery of the mails; on my way to office, I was struck with the magnificent aspect of the sky, which, covered almost entirely with red aurora, presented the appearance of a surging sea of blood. I hastened to see how the instruments were affected, but even though I was prepared for earth currents, yet when I came into office the Morse was working so naturally that for the moment I was deceived, and thought that Kirkwall was calling us, *and went and answered* (as I thought). On taking out the short circuit plug of galvanoscope the needle vibrated right and left. It is one of Henley's single current Morse Embossers, with relay, we use, and the current was so strong that I was afraid it would polarise the cores of electro-magnets, and, therefore, after observing it for a little, I insulated line—the currents were reversed very quickly. As I observed before, I have often seen brilliant aurora, but no currents on lines; in such cases the auroræ, whether in the form of an arch, or simply streamers, have generally been of comparatively steady light, and seemingly some distance off. On the other hand, I have seen strong currents, but no aurora visible, and that too at night, and when the sky was in a condition favourable for observing aurora had there been any, and on such occasions I have seen current here, though none was observable at Kirkwall, and vice versa; moreover I have seen the same with Boddam, a station 22 miles south of Lerwick,—*i.e.*, on ordinary land

wire, and it is well insulated too. Your solution as to probable cause of non-appearance of current at the distant terminal station, has occurred to me when thinking over the subject, and can see through it as regards a current flowing *from*, say this to Kirkwall, and not appearing on Kirkwall instrument, that is, if one may suppose that the accidental current is of much higher tension but less quantity than our battery current, and finds earth over the poles, and faulty insulator of cables, on same principle that a discharge from an electric cloud of sufficient intensity to damage cables would find its way to earth at the lightning protectors. What bothers me, however, is this, if the theory, that these currents are due to the difference of the potential of the terminal earths, holds good, how is it that a current setting *towards* the earth *here*, *i.e.*, from line to earth, does not show at the distant terminal station, where, one would think, they should have greatest effect? I presume that, if two currents sent through the same resistance (say the line from K. F. X.) give the same deflection on galvanometer, they are equal in electro-motive force; then, if so, these accidental currents are often of much greater electro-motive force than our batteries (6, 12 cell Daniell's, connected in single series).

"I mentioned about observing current at *both* ends of our Unst line but not about the centre. Unfortunately I could not get hold of all the stations to learn how the needles were deflected. I observed it both on the 15th and 17th ult.—on the 15th, when working to Cullivoe, he complained that signals were not so good as usual (strong), which I was not surprised at, as my needle was deflected about 45° *left*; on finishing the message I explained the cause, and asked him if his needle was affected, when he informed me it was deflected to *right*. Voe was the only other station I could get at that time, and his needle was deflected left, same as mine. On the 17th my needle was deflected left, at Voe left, at Mossbank slightly left, at Ulsta no deflection, at Linkshouse none, when I asked, but a little right deflection a little before. We have three lines, and I enclose a short description of them and the instruments in use. The Scalloway line (about 6 miles in length) is most affected, indeed not a day passes but there is current on it, more or less, sometimes unworkable, the needle lying right across dial; the Scalloway earth is about a due compass west from here; Baltasound (B. V. F.) is the distant earth of Unst line, and lies about a N.E. by N. from here.

Kirkwall and Lerwick about the same direction. The aurora we see is generally in the north, or extending from east to west across the northern horizon, rarely in the southern half, or if seen there it is all over the sky in every direction. I think that least effect on the wires is when the light or arch is steady, in the *north*, but in most other cases there is considerable effect, and most when the sky is entirely or almost overspread. Just now, whilst I am writing, the needle of Scalloway line is deflected about 70° *right*, on Unst line about 10° *right*, and main line unaffected (the terminals of main line just now are Lerwick and Boddam, cable being useless); now, after an interval of 5 minutes, Scalloway needle returned to zero, and falling to left, vibrating slowly as if a current was surging over line. With reference to snow storms, I have had little opportunity of observing their effects, as we have had comparatively little snow for several years. Of the two or three times I observed effects from snow showers, on one occasion I think the needle was deflected right, but on the other to left. We had one or two pretty thick showers of snow the day before yesterday, but no effect on needles; the snow fell in *small* dry flakes. As I mentioned before, the most marked effect I have seen was with a heavy shower of large fleecy flakes, needle deflected hard over to *left*. I observed this on galvanometer of Morse; it is in circuit *between* the batteries and line, and at that time was connected up and needle so polarised, that, when I put copper to line and zinc to earth deflection was right—zinc to line and copper to earth left deflection. Our needles are connected up with batteries in circuit between coils and line, so that when I put copper to line and zinc to earth through coils the deflection is right, and vice versa; they are not of the old kind of single needles, they are of Reid Bros. manufacture, with right and left keys instead of one handle—simply reversing keys.

"ROBERT STOUT,

"*Postmaster.*"

MAIN LINE.

About 90 miles of land wire and 80 of cable, viz:—Lerwick to Boddam, 22 miles land wire; Boddam to Kirkwall, 100 yards land wire, 60 miles cable, 10 miles land wire, 2 miles cable, 5 miles land wire, 5 miles cable, 4 miles land wire, 1 mile cable, 1 mile land wire.

Kirkwall to Wiek, 15 miles wire, 2 miles cable, 15 miles wire, 10 miles cable, 20 miles wire—I am not very sure, but think these lengths are pretty correct. Instrument at Lerwick, Morse embosser with relay, single current. Instrument at Boddam, Siemens' direct working ink writer, single current.

SCALLOWAY LINE.

Six miles land wire—is most affected with earth current, in fact not a day passes but more or less current—sometimes very strong. Lerwick battery, put copper to line and zinc to earth, deflects needles to right. Scalloway battery, put copper to line and zinc to earth, deflects needles left.

UNST SECTION.

About 62 miles wire and 4 miles cable—

Lerwick to Voe	18	miles wire.	
Voe to Mossbank	10½	do.	
Mossbank to Ulsta, ...	3	do.	and 2 miles cable (Yellsound).
Ulsta to Linkshouse	13	do.	
Linkshouse to Cullivoe ...	8	do.	
Cullivoe to Uyasound ...	3	do.,	and 2 miles cable (Bloomelsound).
Uyasound to Baltasound ...	6	do.	

Eight single needles in circuit—

Connected from Lerwick northwards, right and left.

Lerwick earth on left of instrument.

The coils of all the needles are wound so that a current entering by left coil, and leaving by right one, deflects needle to right and vice versa.

The Chairman remarked that Mr. Stout was evidently a very observant man; and one of the great advantages of this Society, and one of its chief objects, was that it enabled the members to make short communications on any subjects of interest that occurred to them in the course of their studies or labours. It was not always necessary that they should be printed, or even read at the meetings;

but it was very desirable that the example thus set should be followed as much as possible, viz., the sending in of short papers on subjects of interest that occurred to them. There were several questions which one would like to allude to in connection with this subject; but he suggested that they should discuss the whole of the papers together. He would therefore call upon Mr. G. K. Winter to read his paper "On Earth Currents."

The following Paper was then read:—

ON EARTH CURRENTS, AND ON THEIR BEARING UPON THE MEASUREMENT OF THE RESISTANCE OF TELEGRAPH WIRES IN WHICH THEY EXIST.

By G. K. Winter, F.R.A.S., Telegraph Engineer, Madras Railway.

That the rapid progress civilization has made during the last quarter of a century has been aided in no small degree by the Electric Telegraph scarcely requires to be pointed out, while the impetus it has given to the study of electricity, and the assistance it has afforded to its development, can hardly be over estimated.

The Electric Telegraph had been but a short time in existence when it was found that, occasionally, very powerful currents of electricity, from some unknown source, showed themselves in the wires, and very materially interfered with communication. Happily, these disturbances were found to be neither of very frequent occurrence, or of long duration. Subsequently, however, as more delicate instruments were used, it was discovered that the wires were scarcely ever entirely free from them; and though they are rarely strong enough to affect the speaking instruments in general use on land lines, yet they considerably embarrass testing operations.

The Author has for many years given these currents a good deal of attention, and although his experiments are, as yet, very far from complete, he thinks that many of the results already obtained are of some interest, and as the method he has adopted is in some respects novel, a few words on the subject may not be unacceptable.

He commences by assuming that the currents in the wires are due to vast currents, or streams, as he prefers to call them, running through the earth; and that the strength of the derived currents in any one wire is influenced by two circumstances, namely:—

1st. The strength of the *earth stream.*

2nd. The angle its direction makes with the line joining the two earth plates.

Of course the resistance of the circuit, and the distance between the two earth plates, have their influence; their effect may, however, be looked upon as, practically, a constant factor for any one wire; but the variations of the current in the same wire must be attributed to variations in the two conditions above mentioned.

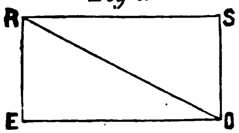
The object of the Author's experiments was to determine these two conditions separately; and to find, if possible, the laws which govern the variations of each.

With only one wire it is impossible to separate the effects due to the two conditions, but by making simultaneous observations on two wires, which, for the sake of simplicity, should be placed at right angles to each other; the separation becomes very easy, and can be made either graphically or mathematically.

The experiments were made at Arconum, near Madras; at which place two wires were erected, each two miles in length, of approximately the same resistance, and one running North and South, the other East and West. The instrument used for measuring the currents in the two wires was a reflecting tangent galvanometer. Attached to the needle and mirror was a stem carrying a small horizontal disc at its base, which disc was suspended in a small vessel of water. In order to eliminate any errors that might arise from change of zero, right and left deflections were taken, and the mean of the two considered the true reading. The current from each wire was observed on the same instrument, and as the whole of the changes were made by a very simple series of switches, and the needle so effectually damped that it went almost immediately to its deflected position, all four observations occupied, in all, less than one minute.

The observations were taken hourly, except on special occasions, and were reduced in a very simple way. The method adopted was simply an application of the principle of the parallelogram of forces.

Fig 1.



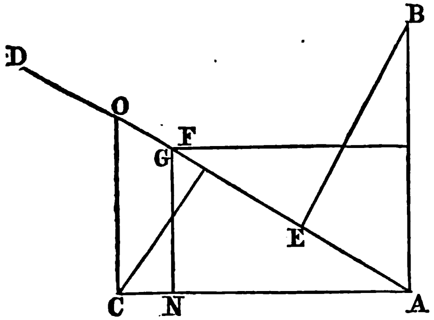
Suppose R O (*Fig.* 1) to represent the direction and strength of the earth stream, and that we have two wires similar to each other, running in the directions O S and O E; then, if through R we draw R S and R E parallel respectively to E O and S O, the lines S O and E O will be proportional to the currents in the two wires.

Again, if we draw O S and O E proportional to the currents observed in the wires running in the directions indicated by those lines, and complete the parallelogram, its diagonal will represent, by its length and direction, the strength and direction of the earth stream.

By resultant current, we mean the current which would exist in a wire, similar in all respects to the experimental wires, but laid in the direction of the earth stream.

The resultant current is by the definition always identical in direction to that of the earth stream, and its strength is, necessarily, proportional to the strength of the stream.

It is really the strength and direction of this resultant stream which is found by the method given in my paper.



Let us suppose A B and A C to represent the two lines at right angles to each other, and A D the direction of the earth stream. Let us suppose that the equipotential lines are at right angles to the direction of the stream, and that the fall in potential down the stream is uniform. From B and C drop perpendicular upon A D, namely, B E and C F—then A E and A F will represent, respectively, the difference of potential between B and A, and C and A. In A D take A G equal A B. Then A G would represent the difference of potential between G and A—draw G m and G n parallel to A C and A B—then G A is the diagonal of the parallelogram G m A n. Through C draw C O parallel to n G.

The triangles C O F and B A E are similar.

C O F, A C F, and A O C are similar.

A O C and A G n are similar.

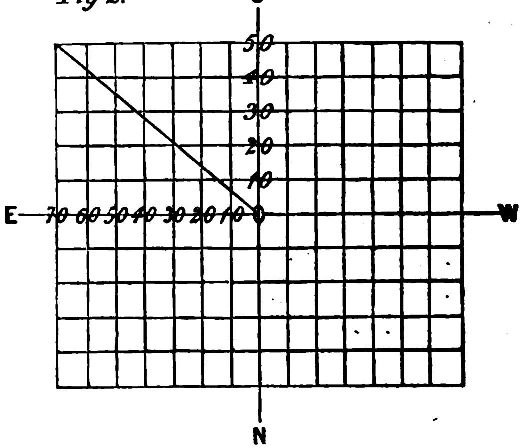
Therefore, A G n and B A E are similar, and being in equal bases are also equal in all respects; so, also, is F A m and A C F. Therefore A m = A E and G m = A F.

But A E and A F are the differences of potential between B and A; and C and A, respectively, and A m, G m are the two sides of the parallelogram whose diagonal is the resultant current. Therefore,

the diagonal of a parallelogram, whose sides represent the currents in the corresponding wires, will represent the strength of the resultant current.

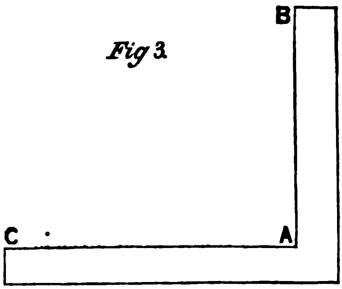
Practically, instead of drawing a figure for each set of observations, a card was ruled as shewn in *Fig.* 2, and the currents observed in the

Fig 2.



two wires were plotted on the lines N S and E W, commencing at O and reckoning in each case toward the point from which the current was flowing. For instance, suppose on the North and South wire a current was observed flowing from the South, and giving a deflection of 50 divisions, and on the East and West wire a current from the East giving 70 divisions. We take the point marked 50 on the line O S and the point marked 70 on the line O E. The point R, where the lines drawn through these points intersect, is at once formed, and the length and direction of the diagonal R O can be easily measured by a scale and protractor. Fine lines, not shown in the figure, were drawn for the units; and to assist the eye in finding the point of intersection, another piece of card, of the shape shown in *Fig.* 3, is used. Its two sides A B—A C—are made to pass through the points on the lines E W and N S, and are kept parallel to the ruled lines; the angle A then gives the point of intersection.

Fig 3.



The Author found from his experiments that the direction of the earth stream varies during the day, but that the daily variation is tolerably uniform. The experiments from which the data are taken were made in December, 1871, and at that time the general direction was from the South; it was most easterly at 5 p.m. and most westerly at 9 a.m., this maximum being very strongly marked. The strength of the current also varied during the day. There was a well marked maximum at 5 p.m. and a minimum at 1 a.m.

The Author regrets that bad health and press of work compelled him to abandon for a time his observations; but he hopes, on his return to India, to resume them with more perfect apparatus, including, if possible, arrangements for automatic registration.

The determination of the laws which govern the variations of the earth stream in different parts of the world would be of vast importance to the science of Terrestrial Magnetism. The Astronomer Royal has had for a long time very perfect apparatus for recording photographically, the currents in his wires; and has shown that a great similarity exists between the current curves and those of the magnetic elements. His experiments, however, are only local; and it is scarcely to be expected that the stream is uniform in its strength, direction and variation in different parts of the World. Apparatus for the measurement of its two elements need not be of an expensive character; and there are, doubtless, numbers of people in different parts of the World quite sufficiently skilled to use it, who would gladly volunteer to do so. Of course it is absolutely necessary, for proper organization, that the matter should be taken up by some scientific body, and not left to individual exertion. Most definite instructions would have to be drawn up, and instruments of one pattern supplied, in order to secure uniformity in the results.

The Author believes the absolute magnetism of the earth to be much more constant than is usually imagined, and that the supposed variation in its elements is due, in a very great measure, if not entirely, to the direct action of the earth stream upon the magnets of the magnetometers. A great deal more might be said upon this interesting and scientifically important subject, but there is another and more practical bearing of the question we have to consider, namely, the effect of the derived currents on our tests, and the best means of eliminating the errors likely to arise from them.

The Author has already treated this question at some length in an

Article in *Phil. Mag.* for March 1872. The subject, however, was far from being exhausted.

It is sometimes supposed that if the Earth current be constant the mean of two ordinary bridge tests, one with copper to line, and the other with zinc to line, will give the true resistance of the wire, and that if the earth current is, as it usually is, variable, the mean of several tests, with alternately copper and zinc to line, will be sufficiently accurate. For the sake of simplicity we will first consider the case of a constant earth current, or rather a constant difference of potential, between the two earth plates.

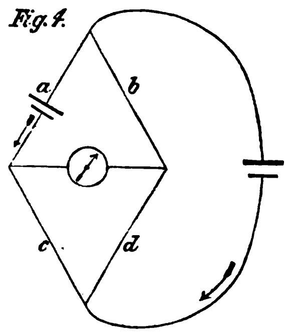
If this difference of potential is small compared with that of the testing battery, and, consequently, the readings not very different from each other, the mean of the two readings may be taken as approximately correct; but if the readings differ largely from each other, the earth current is very considerable, and the mean of the two readings is very far from the truth.

Before we proceed further, it will be well for us to understand clearly the reason for the fact just mentioned; and in investigating the subject we shall doubtless see some way of removing the difficulty, if it should be considered necessary to do so.

We must consider the earth current as due to an electro-motive force in one of the branches of a bridge.

Fig. 4 represents the bridge with an electro-motive force in the branch *a*. Let us, for simplicity, suppose that all the branches are equal; of course the earth current will produce a deflection on the galvanometer, that is, it will cause a difference of potential between the two terminals of the galvanometer.

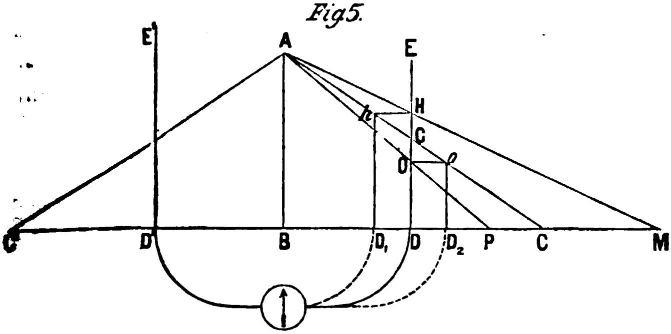
Fig. 4.



It is evident that, having applied the testing battery, we can neutralize this deflection by so altering the balance, as to tend to produce an equal and opposite difference of potential between the galvanometer terminals. Let *b* be the adjustable resistance, then if the direction of the testing current be that indicated by the large arrow, and that of the earth current that indicated by the small arrow, we shall have to increase *a* in order to produce a zero reading of the galvanometer. If either the testing battery or earth current be reversed, we shall have to decrease *b* in order to produce the same effect.

In practice it is the testing battery which is reversed, but it will simplify matters, and come to exactly the same thing, if, instead of supposing the testing battery reversed, we suppose the earth current to be reversed, without suffering any change in its electro-motive force; so that it may cause a difference of potential between the terminals of the galvanometer, equal in each case, but opposite in sense. The problem then resolves itself to this. It is required to produce a given difference of potential between the terminals of the galvanometer, first by increasing *b*; and, secondly, an equal and opposite difference of potential by decreasing *b*. We may for the moment disregard the earth current altogether.

Fig 5.



Let A B, *Fig.* 5, represent the difference of potential between the battery contact points, caused by the testing battery. Let A C and A C′ represent the fall in potential on each of the sides of the bridge B D, D C and B D′, D′ C′ representing the four branches; and D D′ the galvanometer terminals. Through D and D′ draw D E and D′ E′ perpendicular to C C′, cutting A C and A C′ in G and G′. Then D G and D_1 G′ will be equal, that is, the potential of H at the two terminals will be equal.

First, let us make the potential at D′ higher than that at D by an amount equal to G H. Join A H and produce it to meet B C, produced in M. Then C M will be the resistance to be added to D C in order to cause a difference of potential, equal to G H between the terminals; that at D being higher than that at D′.

Secondly, let us make the potential at D lower than that at D′ by an amount equal to G O which is equal to G H. Join A O and produce it to meet B C in P. Then C P will be the amount to be subtracted from D C in order to produce this second difference of

potential between the terminals, which is equal to the former difference, but opposite in sense. Then D P and D M will be the two readings of the adjustable branch, and it is very evident that the mean of the two is not D C, the true resistance, which would produce balance under ordinary circumstances.*

This simple way of looking at the question leads at once to the suggestion that if we left the resistance B C constant so as not to interfere with the fall of the potential A C′, and simply produced the required difference of potential betweeen the galvanometer terminals by moving D along B C to D_1 in the one case, and to D_2 in the other; we should, without sacrificing the principles of the bridge, get two readings, the mean of which would enable us at once to calculate the true resistance; for D is evidently midway between D_1 and D_2; this is supposing, as we have hitherto tacitly done, that the resistance of the testing battery is small enough, compared with the other resistances in the circuit, to be disregarded.

Fig. 6.

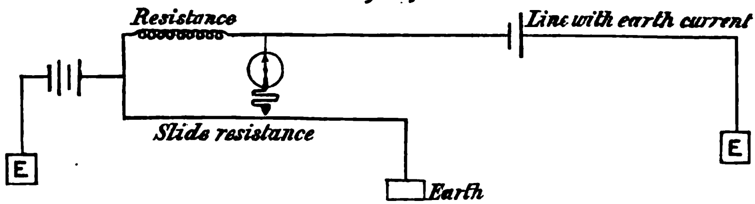


Fig. 6 shows the application of this idea to practice; there are many devices by which the sliding contact could be made.

So much for a constant earth current; it is however the variation, and not the existence of the earth current, that is the source of our chief difficulty; for as both the electro-motive force and resistance are unknown, we require two observations to enable us to determine either; and it is difficult to take two observations so quickly one after the other, as to ensure constancy of the earth current during the interval.

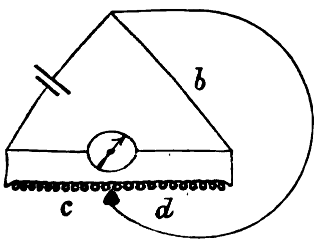
Mr. Mance described a method, and the author has pointed out another, in which one observation is merely noting a deflection; and the second, seeing that this deflection remains the same, after making

* There is a small influence tending to diminish this error, due to the decrease in the earth current caused by increasing the resistance of the adjustable branch, and the increase of the earth current due to the decrease of this resistance; thus less resistance need be added in the one case, and rather more must be subtracted in the other, than if the earth current were unaltered by the adjustment. The formula given by Schwendler; and the Author, of course, takes this effect into consideration as well as that given in the text.

or breaking contact with a key. The object of each method is to reduce the interval between the observations to the least possible quantity, so as to give the earth current no time to vary. In each method the line, with its two earth plates, is treated as a rheo-motor, and its internal resistance is measured.

Mance's method depends upon the fact that, if the resistances in the four branches are properly proportional, so as, under ordinary circumstances, to produce a balance, and if a current is generated in one, so as to produce a deflection on the galvanometer, we may join the two points where the battery is usually applied without altering the deflection. In order to avoid producing any variation in the deflection by the adjustment of the balance, he alters the ratio of the proportional resistances without altering their shunting power. This is done, as in *Fig.* 7, by the point of contact being variable, so that the sum of the two resistances, *c* and *d*, is always the same. The resistance *b* is, of course, kept constant.

Fig. 7.



The method proposed by the author was described in the paper already referred to, and is simply a modification of Varley's method of measuring the resistance of a cell by means of the differential galvanometer.

Years ago the author, in taking conductivity tests with the differential galvanometer, took the deflected position as the zero, everything else being the same as in ordinary resistance testing; and Mr. Saunders, of the Red Sea line, states that he has done the same with the bridge. Both these methods depend on the same principle as the two above referred to, but they are, in some respects, not so satisfactory. The deflected position has to be read, and the galvanometer put on short circuit, while the battery is applied; or the sudden flow through the galvanometer, in testing cables, would alter the magnetism of the needle.

The difficulty as to the sudden flow through the galvanometer is common, but in a greatly diminished degree, to both Mance's method and the Author's, when these tests are applied to cables, or even to long and well insulated land lines, such as those in India. In these cases the flow is not sufficient, usually, to necessitate the short

circuiting of the galvanometer; but still it disturbs the needle, and obliges us to wait until it comes to rest before the second reading can be taken, in the meantime the earth current may have altered. The author proposes to remedy this in two ways, but at present he only deals with one, namely, the use of a condenser.

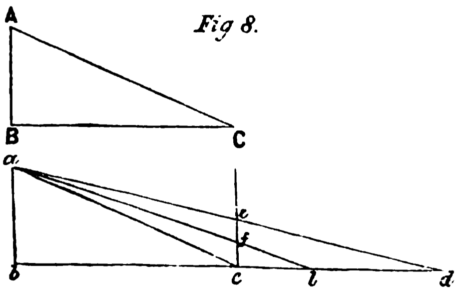
Of course the same plan may, with modifications, be applied to other methods of measuring resistance, but we will consider it specially in its application to Mance's method, because it will enable us more easily, to notice some interesting points connected with earth currents, that may not be generally recognized.

Suppose a cable to be laid between two points in the earth, between which there is a great difference of potential; and further suppose, that the fall from one point to the other is uniform; and that the conductivity of the conductor is uniform also; then if the cable be connected to earth at each end, there will be no charge in the cable, for at each point in its length the potential of the conductor inside will be the same as that of the ocean outside, no matter how strong a current may be, at the time, flowing through the cable, in consequence of such difference of potential between the points in the earth, between which it is laid.

If, however, a resistance be inserted at one end, there will be a charge which will increase (but not in direct proportion) with the resistance. When the resistance becomes infinite the charge reaches a maximum, which maximum charge will be half that which would be caused if a battery, equal in difference of potential to that between the two points in the earth, were applied to one end of the cable, the other end being insulated.

Now when the resistance of the testing instrument is inserted between the cable and the earth at one end, we immediately get a slight charge in the cable; if we suddenly change the resistance of the instrument we alter the amount of charge, and a portion of the difference will enter or leave the cable through the instrument, according as we increase or decrease its resistance.

Fig 8.



Let us suppose B and C (Fig. 8) to be the earth at the two ends of the cable, and that there is a difference of potential, equal to A B, between them. And further, that the fall from B to C is uniform, and may be represented by the straight line, A C.

Next let *b c* represent a cable, laid from B to C; and first, let us suppose it to be joined to earth at each end. Then *a b* equal to A B, will be the difference of potential between its ends; and the fall of potential, *a c*, will be exactly similar, and parallel to A C, that of the earth between the two places; and, no matter how great the difference of potential between B and C, there will be no charge in the cable; for at each point the potential, each side of the dielectric, will be the same.

Now, considering the resistance of the cable to be *b c*, let us insert a resistance at *c*, equal to *c d*. Join *a d*. Then *a d* will represent the fall in potential in the cable and resistance. Through *c* draw *c e* perpendicular to *b c*, cutting *a d* in *e*; then *c e* will represent the difference of potential between the end of the cable and the earth at *c*; and the charge in the cable will be represented by the area of the triangle, *c a e*. Let us now suddenly reduce the inserted resistance to *c l*; join *a l*, cutting *c e* in *f*. Then the charge in the cable will be reduced to *c a f*; and the difference, *f a e*, will suddenly flow out, part at each end.

Let us now apply this reasoning to Mance's test.

Fig. 9.

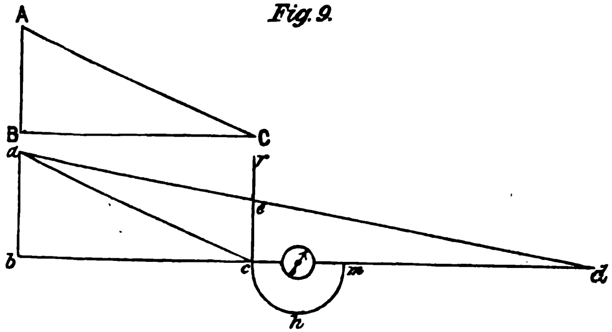


Fig. 9 represents the state of things before the sliding contact is made. In this diagram, as before, B and *c* are the earth at the cable ends; A B and *a b* the difference of potential between them; *b c* the resistance of the cable. Let *c m* be the resistance of the galvanometer shunted by the slide resistance, *c h m*, and *m d* the fixed

resistance. Join $a d$; and through c draw $c e$ perpendicular to $b d$, cutting $a d$ in e. Charge will equal $c a e$. Let us first suppose that $a c = m d$.

Then Mance has shown that, if we put a point, h, in the centre of the shunt, to earth, no alteration will be produced in the strength of the current passing through the galvanometer. But in putting h to earth we alter the resistance of the testing instrument, and consequently the charge in the cable.

Fig. 10.

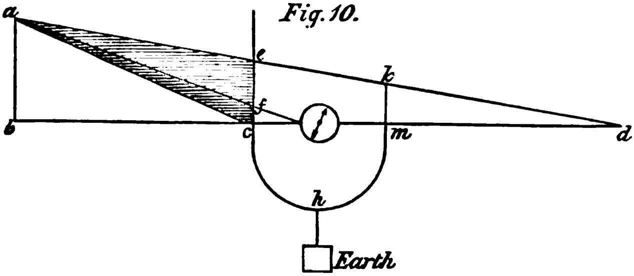


Fig. 10 will show the state of things when contact is made, if we suppose that by doing so $c d$ is reduced to $c l$. Join $a l$, cutting $c e$ in f. Then the area of the dark triangle, $c a f$, will represent the charge remaining in the cable; and the light triangle, $f a e$, will represent the discharge, part of which will flow out at b, and part at h. It is this discharge which causes the sudden jerk of the needle; for a portion of it runs through the galvanometer to m, and down to h; and a small quantity on to d. The author proposes to remedy this by converting $m d$ into an artificial cable, so that a simultaneous discharge may take place from $b c$ and $m d$; and consequently neutralize each other's action on the galvanometer.

Through m draw $m k$ perpendicular to $m d$, meeting $a d$ in k. Then, before h is put to earth, $m k$ will represent the difference of potential between m and d. Let us attach such a condenser to $m d$ that, with this difference of potential between its armatures, it will contain a charge equal to that represented by the light triangle, $f a e$. Now, when h put to earth, an equal quantity of discharge will tend to pass from m to c as from c to m, and consequently the galvanometer will be unaffected.

In order to avoid having to use too large a condenser we may make $m d = 10\ b c$ and $m h = 10\ h c$; when, of course, a very much smaller condenser will be required.

There is another and still more important subject connected with earth currents on which the author hopes to be able to say a few words before returning to India; this is the effect produced by these currents on our signalling apparatus, and the best means of counteracting it. In the meantime he hopes that the means he has now pointed out of obviating some of the difficulties in testing the resistance of telegraph wires, in which these currents exist, may be of some value to telegraph engineers.

And now, as to the more scientific side of the question first considered: the author has said that it should be taken in hand by some scientific body. Now it would, he thinks, be difficult to find a scientific body better qualified to take this matter in hand than the Society of Telegraph Engineers.

No society in the world is, he should say, so largely or so widely represented by men accustomed to the use of electrical instruments of precision than our own; and he sincerely hopes the time is not far distant when it may with truth be said that the Society of Telegraph Engineers had laid the foundation of a new and most important branch of the Physical Geography of the earth.

The author has already given it as his opinion that the magnetism of the earth is much *more constant* than is usually imagined, and that the *movements of the magnetometers* are due chiefly, if not entirely, to these earth streams.

We have no direct evidence that the currents themselves are due to changes in the earth's magnetism, while we have much that points to the sun as their origin. It is most probable that they are due to the direct action of induction of exterior bodies upon the earth, acting as a moving conductor. In considering the earth as a moving conductor, we must consider its conductivity to be influenced by its temperature, hence, probably, the effect of changes of weather upon the currents which has been noticed by more than one observer. The influence of solar phenomena upon magnetic storms has long been known, but we shall learn little more than we already know on the subject, while we look upon these storms as merely changes in the terrestrial magnetic elements accompanied by earth currents. To know more, we must go to the cause of the movements of the magnetometer, and the author feels confident that results of great importance are sure to follow the investigation.

The Chairman said, before proceeding to discuss these interesting

papers, the Secretary would read a communication from Mr. James Graves, of Valentia, on the same subject, after which they would proceed with the discussion.

The following paper was then read by the Secretary:—

ON EARTH CURRENTS.

By James Graves.

In long submarine cables, such as those across the Atlantic, there is at all times present more or less of what is commonly called earth currents; or there is always a difference in the electrostatic tension of the earth's crust at the two extremes of the cable, giving rise to a dynamic current through it upon putting both its ends in connection with the earth.

This current flows sometimes in one direction, and sometimes in the contrary direction—it being sometimes positive, and sometimes negative—but in neither direction does it flow for many hours together without changing to the opposite sign, and very seldom does it remain for many minutes or even seconds of the same force in any direction, as it is constantly varying, and this renders the study of it very difficult.

Although earth currents, so called, arise chiefly from differences of potential at the extremes of the cable, they are doubtless also caused by difference of temperature giving rise to a thermo-electric current; and probably by geomagnetic lines of force darting from pole to pole, or from equator to pole, and vice versâ, across the line of cable, inducing a magneto-electric current in the insulated wire.

Laws are the offspring of facts, and not facts the results of laws, whilst facts are built upon accurate observation. The part I have proposed to myself in this interesting subject is to bring before your notice observations which no one else has ever had the opportunity of making, and to submit them to the talented members of this Society in the lively hope that they may, by some one, be moulded into a fair and comely form.

Unfortunately, as is too well known to most of you, for some months in the year 1871, *both* the cables of the Anglo-American Telegraph Company were *hors de combat.* During this interval

when, despite all that has been said about working with naked wires, or even without wires at all across the Atlantic, these cables with their broken ends only a few yards apart refused to speak, I embraced the opportunity—simultaneously thought of by a distinguished electrician amongst our number—of recording, every half hour, the nature and strength of these earth currents in the two cables; the length of one being about 1,850 nautical miles, and that of the other about 1,820 nautical miles to their broken ends, the copper conductors of both being exposed to sea water.

The records of these observations were, for the two cables, practically so similar that it is only necessary to deal with one of them to arrive at a general result. For this purpose I have chosen the longer of the two, the 1865 cable, whose length, as just stated, was 1,850 nauts.

Night and day, for 78 days, the force and direction of the earth currents were observed and recorded. These recorded deflections I have worked out into their relative values, taking one of our ordinary working battery cells as a unit of force, and have put them into a tabulated form, showing the strength and direction at each half hour during the whole period of 78 days.

To reproduce these values in the shape of curves for *each day*, would greatly add to my already very extensive work. I have, therefore, contented myself with drawing the curves of eight of the days when extraordinary influences do not appear to have caused any palpable irregularities, and from these on sheets 1, 2, 3 and 4, it will be seen that there is a very remarkable similarity in their general contour, which would tend to show that, when not interfered with by any extraordinary meteorological, or magnetic disturbances, there are two maxima positive and two maxima negative per diem, that is to say, a first maximum positive is reached about 3 or 4 a.m.; a first maximum negative about 7 or 8 a.m.; a second and larger maximum positive about 12 or 1 p.m.; and a second and larger maximum negative about 6 or 7 p.m.

At the foot of the large sheet, No. 6, containing all the readings for the whole period, will be found the total number of days, for each half hour of the day, when the current was observed to be (+) positive, (—) negative, and (‡) neutral.

By taking the difference between the number of positive and negative readings for each half hour a majority is obtained in favour

of either the one or the other, and on sheet No. 5 I have given both the number of each and a curve of these *differences* which, totally irrespective of its force, gives a general mean of the *nature* of the current on an average of the whole period.

I have further taken the time of mean sunrise and sunset between the 6th of March and the 22nd of May, and inserted them in sheet No. 5, which, by comparison with sheets No. 1, 2, 3, and 4, will enable one to see at a glanee how much greater effect the sun has upon the strength of these currents when *above*, than when *below* the horizon.

The curve of differences on sheet No. 5 will also be found to correspond very remarkably with the mean curvatures of the eight days whose curves I have drawn.

A few words in explanation of sheet No. 6 may not be out of place here. During the former portion of the period shunts were not employed, but when the spot was beyond control, it is represented by (>20) greater than 20 cells, but during the latter part shunts were introduced to keep the spot within easy range. All these readings have been carefully calculated, and where the force was below 20 cells it is given, and where above 20 it is entered either as the actual force or as (>20) greater than 20 cells. I have summarised these latter forces, and the maximum observed at any time was equal to 112 cells + at 10 p.m., April 9th, an aurora being visible the same night.

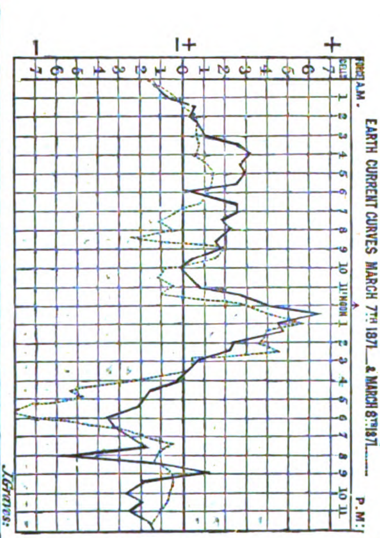
From the curve of differences it will be observed that the majority of positive currents reaches its lesser maximum at 3 a.m., and its greater maximum at 12 noon; and that the majority of negative currents reaches its lesser maximum at 8.30 a.m., and its greater maximum at 4.30 p.m. Generally speaking a *large* rise and fall takes place while the sun is *up*, and a *smaller* rise and fall while he is *down*.

The times of change are as follows:—

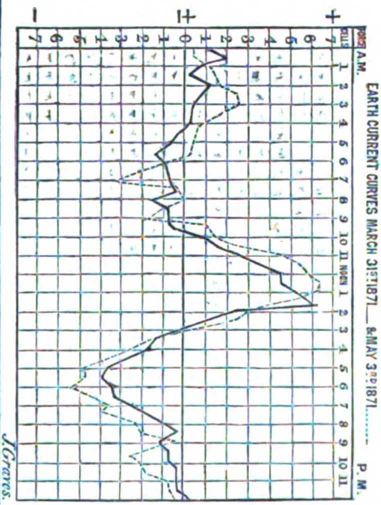
From Positive to Negative (+ to —) about 6.30 a.m.
„ Negative to Positive (— to +) „ 9.30 a.m.
„ Positive to Negative (+ to —) „ 2.30 p.m.
„ Negative to Positive (— to +) „ 10.30 p.m.

I should remark that the time given throughout is Greenwich mean time.

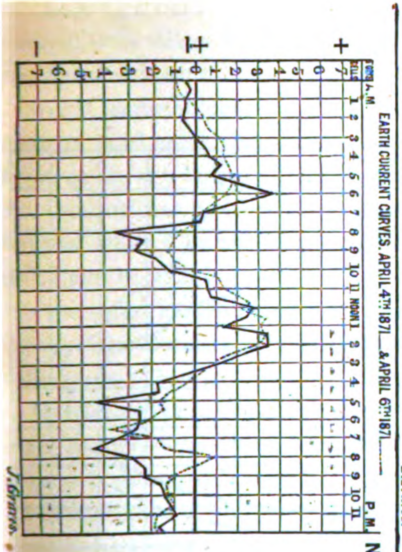
Of course anything tending to neutralise, or to increase, the force



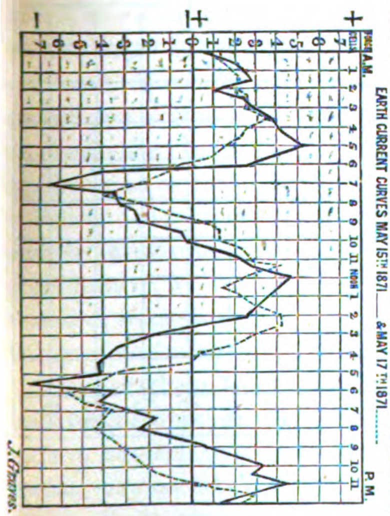
No. 1.



No. 2.



No. 3.

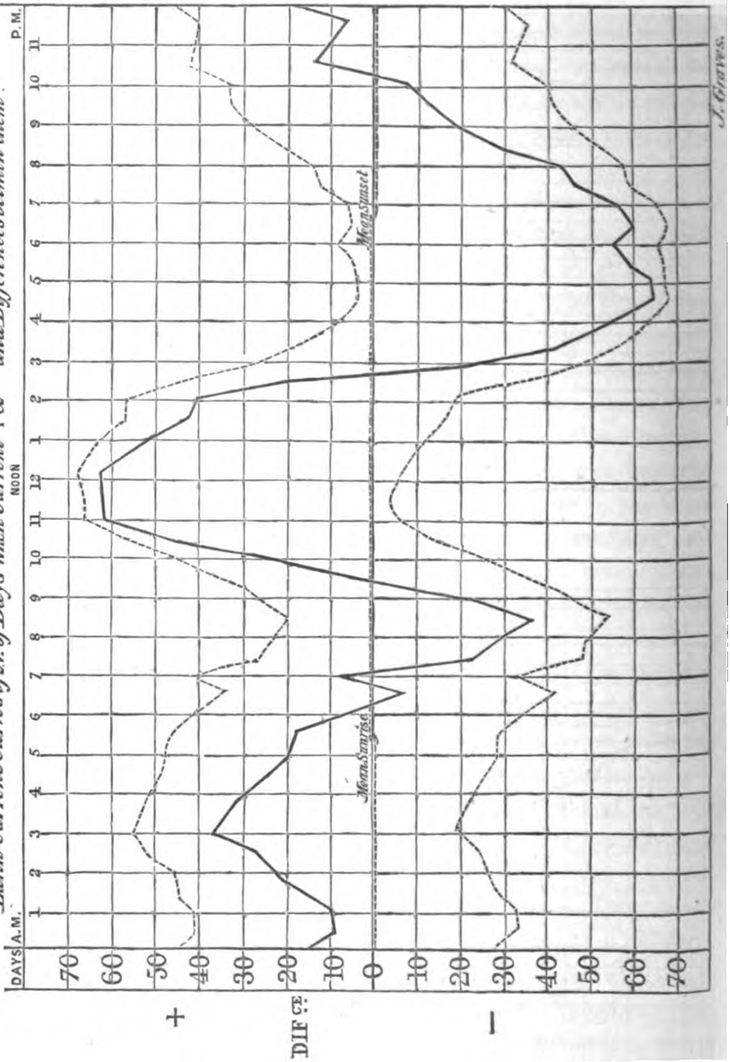


No. 4.

No. 5.

MARCH 6TH 1871 TO MAY 22ND 1871.

Earth Current Curves of No. of Days when Current + & − and Differences between them.



of these currents temporarily, will produce variations in the daily curves, still these inequalities are easily recognised apart from the regular daily curve.

Great meteorological changes, either from fine to stormy, or from heat to cold, and vice versâ, as well as sudden changes of wind in force or direction, are generally accompanied, or immediately followed, and sometimes preceded by an earth current of unusual force. I have in sheet 6 inserted, as "remarks," such meteorological phenomena as I have been able to observe or collect, but other coincidences with the recorded irregularities may possibly be obtained from meteorological records elsewhere, as influences, far from local, frequently affect submarine cables.

More learned heads than mine may be able to show the connection more accurately between the diurnal earth current variation and the solar orb due possibly to the relative position of this planet's axis in relation thereto; but, that these earth currents have their periods of duration and change, is, I think, beyond dispute.

Whether or not they are controlled in any way by the relative positions of the earth and the sun, I am not prepared to *prove*, and I must leave that to our astronomers and others to elaborate into a law; but I think I have sufficiently shown that the sun, when in the cable's meridian (or a little before it), has the effect of producing a positive earth current; greater when on the same side of the earth, and lesser when relatively under the cable having the section of the earth between them: and, further, that intermediately between these two periods a negative current intervenes, greater at or about sunset, and lesser at or about sunrise.

When the meteorological conditions are such as to produce an auroral display in these latitudes, this phenomenon is invariably accompanied by extraordinary earth currents, generally rapidly reversing from + to — and vice versâ, a great argument against the theory of the aurora being produced by reflection from polar ice.

Whether these aurora are produced by strong inductive action between the upper rarefied stratum of our atmosphere and the earth's surface, rendering itself visible to our senses on the principle of the partial vacuum tubes, which appears to be a near approach to the nature of the phenomenon; or by violent disturbances between the magnetic poles and the magnetic equator, projecting luminous curved lines of force in all directions radiating from a common centre at the

poles; or whatever may be the *cause* of the phenomenon, there is, I think, sufficient proof that that *same cause* is constantly operating in the production of earth currents, and that, it is *only* when that cause, and its effects, are highly intensified by solar, and it may be, by planetary influences, the fact is made visible to us in the shape of an auroral display.

Full detail of the great magnetic storm of 4th February, 1872, have, I observe, been collected by our present secretary, and published by the society.

Bearing upon the same subject, it may perhaps be worthy of remark, that whenever an earthquake is experienced in any part of the globe, the telegraphic lines are more than usually influenced by earth currents. I have noticed this on several occasions, but I did not, till recently, take the precaution to preserve the newspaper slips containing the reports and the dates.

In sheet No. 6 a "remark" is made for 17th March, when most violent disturbances occurred on the cables, to the effect, that an earthquake was felt in the North of England. The newspaper reporting this, stated that it was first felt between 6 and 7 p.m., on the 17th; at Preston it was felt at 11.5 p.m., and at Hexham at 11.15 p.m. It was not felt in Scotland, north of Dumfries. The effects reached the Atlantic Cables at midnight of the 17th, and caused great irregularities for seven hours afterwards.

The following extract was copied by *The Times* from *Allen's Indian Mail*:—

"Earthquake in India. From the Bombay papers we learn that a "severe earthquake happened along the Scinde Frontier on the 15th of "December, 1872. The worst of the shock seems to have been felt "in the Kachi District, 200, or, according to another account, 500 "lives have been lost in two villages alone. Only 12 persons were "saved amid the general wreck. At several other places, such as "Shaikarpúr, Schwán, and Rori, the shocks lasted from one to five "minutes, but did no particular damage."

This shock, felt in India so severely, on the 15th December, was preceded by strong earth currents which necessitated the looping of our two land-wires between Valentia and London, during all the latter part of the evening of the 14th December.

The following extract is from *The Times*—

"Earthquake in Egypt. The Baron de Cosson, writing to us from

"Cairo, says, that a shock of earthquake was felt by many persons "there on the 12th of January, 1873, at 1.49 p.m. The oscillations "which, as far as our correspondent could observe, were from "south-west by west, to north-east by east, lasted about a minute and "a half. The movement resembled that experienced on a very "small boat in a little chopping sea."

This shock was felt at Cairo between 1 and 2 p.m. on 12th January, and the land lines between Valentia and London were so disturbed that, from 10.35 p.m. of the 11th to 12.35 a.m. of the 12th, the two wires were looped to be able to work at all. This earthquake, like the previous one in India, was preceded for some hours by an extraordinary earth current.

The following extract is from *The Times* of 9th April, 1873. An Icelandic Volcano—

"Eruption of Shaptar Jokull, Iceland, from 9th to 12th of January. Seen from most parts, magnificent appearance."

The telegraphic lines were influenced by deflections all night of 9th. Currents on lines all day 10th. Deflections in evening of 11th, and also up to 12.35 a.m. of 12th, as referred to above as coincident with the earthquake in Egypt.

A line, drawn direct from Cairo to Iceland, crosses the lines of telegraph between England and Ireland.

These coincidences, with others which I have noticed, but of which I have no record, show that great internal convulsions of the earth give rise to electrical currents of unusual intensity, and that the electric equilibrium of the various portions of the earth's crust, however far apart, are liable to be disturbed by the same cause; this disturbance becoming visible through the medium of the earth plates and their intervening connected lines and cables—yea, they would almost warrant us to predict that, whenever unusually strong currents interfere with our working, something is happening to our fellow beings, or their property, in some quarter of the globe, or at sea, where it is least likely to be observed and recorded by anyone; and I have no doubt, now that our cables are fast encompassing the globe, enabling us to observe that phenomenon, that we shall be able to trace, for every earthquake recorded, a corresponding interference with some one or more, if not with all, of our long telegraphic circuits.

There is one obstacle, however, which will seriously militate against

the observation of these disturbing influences upon long cables, and that is that, in order to work them at their greatest commercial capacity, we institute a system of signalling which is designed specially to obviate the disturbing influences of these earth currents, and thereby we render the observation of them almost impracticable for scientific purposes, insomuch, that the land circuits are far more troubled with them than the cables.

This fact should make the records I now present to the Society all the more valuable, as I hope such an occasion will never again occur as that during which they were obtained, and it has been this feeling which has prompted me to impose upon myself a task, which has occupied much of my little leisure during many months.

I have already extended this paper far beyond the limits which I originally intended, and I must, therefore, reserve for another essay, some further remarks which I have to make upon the *effect* of these earth currents upon the results obtained when testing for the conductivity of the conductors of long submarine cables.

TABLE OF FORCES OF EARTH CURRENTS EXCEEDING 20 CELLS.

	Cells.		Cells.		Cells.
April 1st, 1871	— 34.97	April 10th, 1871	+ 43.64	April 24th, 1871	+ 30.75
	— 48.85		+ 26.88		+ 30.75
	— 23.60		+ 26.63		+ 47.56
	+ 24.72	April 15th, 1871	— 26.80	April 27th, 1871	— 20.10
	+ 26.90		— 23.90		— 26.63
	— 20.20		— 38.04	April 28th, 1871	— 41.44
	— 24.21		— 28.46		— 31.63
	+ 56.95		— 26.09		— 25.00
	+ 26.38		— 35.55		+ 23.96
	+ 42.26		+ 37.21		+ 20.43
April 2nd, 1871	+ 20.32		+ 30.81	April 29th, 1871	— 20.7[illegible]
April 9th, 1871	— 37.60	April 23rd, 1871	— 27.75		+ 20.[illegible]
	— 34.10		— 32.49		— 30.52
	— 53.20		— 29.38		— 47.56
	— 54.30		+ 26.10		— 35.10
	+ 35.40		+ 34.80		+ 32.85
10 p.m.	+112.00	April 24th, 1871	— 24.18		+ 34.48
	+ 56.00		— 26.63		+ 38.76
	+ 84.00		— 31.63		+ 33.29
	— 27.10		+ 47.56	April 30th, 1871	+ 26.20
April 10th, 1871	— 29.21		+ 22.84		+ 30.75
	— 24.60		+ 26.63		

See chart in pocket at back of book

THE CHAIRMAN said the diagrams which accompanied the above communication were very interesting, and showed, in the clearest manner, the definite diurnal variations of the forces of earth currents, as exhibited in the Atlantic Cable. He would now call upon gentlemen to discuss the three communications before them.

Mr. C. F. VARLEY said it was rather a large task to undertake to discuss so many papers without previously reading them, but fortunately it was a subject with which he was pretty familiar. A distinguished member of this Society, Mr. Barlow, was the first person who made a systematic record of earth currents, and he began his work in 1846-7, assisted by one of their vice-presidents, Mr. Culley. Mr. Barlow observed these earth currents, and plotted down their strength night after night for a long period, and those records would be found in the Transactions of the Royal Society. Those observations were made upon the telegraph wires of the Midland Railway, and it was found that the strength of the currents was by no means proportional to the length of line in circuit, but it depended upon the direction of the line whether it coincided or not with the direction of the earth currents which produced the phenomena, and Mr. Barlow, moreover, found there were periods of maxima and minima, in fact, that there were always earth currents, and that the maxima and minima varied with the time of year. Besides these daily currents of feeble strength, they observed very powerful irregular currents whenever there was a display of the Aurora Borealis. These records were made with great skill, and, considering the imperfect appliances at command, these results did great credit to those who arrived at them. In 1847 he (Mr. Varley) observed the first auroral current it fell to his lot to see, and that was on the occasion of the tremendous auroral display, in which the auroral arch was distinctly visible above the horizon. He was at Dawlish, in South Devon, when this occurred. On that occasion all the telegraph lines in operation in Great Britain were stopped for the time being by these currents. He thought it might be said that that was the first time attention was fully drawn to the subject, and it was taken up not only by the officers of the telegraph company, but also by the Astronomer Royal. Shortly after this, Mr. Walker, who had made a great many observations upon earth currents in conjunction with the Astronomer Royal, erected two circuits for the Royal Observatory, at right angles one to the other. For twenty years or more the currents on these

two circuits have been photographically recorded, and the tr direction of the current through the earth approximately determin therefrom; but in the early observations the relative resistance each circuit had not been taken into account. If an earth-plate of t limited size be used, the resistance of the current will vary a gre deal; and if they attempted to get a very large earth-plate, by co necting their wires with a system of gas or water pipes, then t current observed was due not only to that portion of the earth includ between the earth plates, but also to the system of iron pipes on o side and the other. In 1853 or 1854, the earth current on a length two miles of wire between Telegraph Street and the Strand was s strong that it was impossible to work the line during its continuance He was astonished at the enormous strength of the current on s short a length. No doubt that current was not an earth current be longing to a difference of potential of two miles, but was owing t the pipes belonging to two different gas companies, one in Telegrap Street and the other in the Strand, and the telegraph instrument indicated the electricity brought to them by those enormous con ductors. On the South Devon Railway, in 1847, he observed a earth current between Dawlish and Teignmouth, a distance of thre miles. The only battery at command was an ordinary copper and zinc plate sand battery, such as were then generally employed. Those who were accustomed to work that battery knew that the potential of such a battery was liable to enormous variation; but he found 24 cells of such a battery was insufficient to balance the earth current over that line of three miles only. More recently, and since 1852 more careful observations had been made in Telegraph Street of the principal earth currents that had occurred. These he communicated to the Astronomer Royal, who took very great interest indeed in the subject. Many of these had been published, and some curious results had been obtained. In the first place he had found that there were certain localities in which the currents were strongest, and this wa particularly the case between Ipswich and London. Ipswich migh be considered as an indentation of the coast; the sea came in a funne and the telegraph wires connected that funnel with London. At King' Lynn they had a similar funnel, not far north of Ipswich, yet betwee London and Lynn the currents were feeble. The circuit betwee London and Newcastle, which was north of the Lynn circuit, behave like the London and Ipswich circuit—it gave powerful current

almost invariably in the same direction as the London and Ipswich line. He made many attempts to find the direction of the neutral line line of equal potential. It soon became apparent that this neutral line approximately coincided with the coast line, and that was most likely due to the circumstance that sand and arable land, as compared with sea-water, were poor conductors, and consequently the earth currents on these particular land lines were the currents brought by the sea, partly arrested by the less-conducting land, and carried by the telegraph wires. Therefore, in the observations made of these currents, it would not be fair to infer that the general current of the earth was of the strength indicated on any one line, from the fact of the great currents being arrested by the less conduction of the land. The currents between Ireland and Newfoundland almost in the same line as from Ipswich to London, were but little stronger than the currents from Ipswich to London. In his communications on this subject with the Astronomer Royal, he pointed out how valueless, comparatively, were those observations to determine the true flow of the currents in this island, and he suggested that at every observatory there should be cross wires laid down, 6 or 7 miles in length. He had himself endeavoured to measure earth currents on short wires. In his garden he had wires varying from 200 to 600 yards, but he found the variations due to different states of moisture, and other conditions were so great, that the direction of the earth currents could not with certainty be deduced therefrom, in fact he knew nothing more difficult than to get two earth-plates possessing equal potentials.

In the first paper read this evening, attention was drawn to the fact that the earth current varied very much during snow storms. He was informed that Mr. Stout was mistaken with regard to the change of the earth current from snow—that he was mistaken in the case which he observed. It was most likely he thought what he had observed had been due, first, to the snow producing leakage between his wires and the earth. The rapid variations of earth currents which Mr. Stout described, must be due to some station endeavouring to call, and not due to the earth itself, for he had never yet, on any occasion, found an earth current to change from maximum positive to maximum negative in less than 30 seconds: more generally it occupied a period of several minutes, and on some occasions he had seen a continuous current in one direction for 20 minutes. In November and December, 1867, he took a number of observations on the Rigi,

to see whether at such an elevation, where he was comparatively free from the influence of surrounding mountains, there was such a state of atmosphere as had often been described, viz., a negative cloud. He once during the 14 days he was there observed a negative cloud of 400 Daniel cells during a snow storm, but the atmosphere was only negative for a period of an hour and a-half, and then resumed its normal condition. In that case the change was gradual; it occurred during a snow storm. He did not draw any inference on account of the snow storm, but he mentioned the circumstance so that if any one had an opportunity of making similar observations, it would be interesting to know whether the deposition of crystals of ice from water vapour, did influence or not the electrical condition of the atmosphere. The question of earth currents in long cables had often been one which had caused him very serious doubts as to their longevity. When the 1865 cable was broken, just after 1,200 miles had been laid, it would be remembered a series of very powerful earth currents showed themselves, and Mr. Graves, who was observing at Valentia, stated that such was the power of the currents that a distinct arc of flame burned between the key and the earth connection.

Mr. Varley estimated the electric potential necessary to produce such a flame at not less than 2,000 cells of Daniell's battery. He had seen between Ipswich and London a difference of potential of from 70 to 100 cells of a battery, while in this case it was not less than 2,000. In America, on long circuits, there had been observed earth currents of greater power than this, but as he had said, the currents on land lines were always more powerful, because of the obstruction which the land offered to the passage of the current.

Another interesting result, which came from the observation of these earth currents, was that when the late Admiral Fitzroy was establishing his weather signals, in which the Electric Telegraph Company rendered him great service, he (Mr. Varley) had observed on seveal occasions that the earth currents were followed by a change of weather. He communicated that fact to Admiral Fitzroy, and continued to send him for a short time notice of the earth currents; and he found the information of so much assistance to him in predicting the coming of storms, that he requested to have it regularly supplied. On some occasions he could see the approach of a storm days before the barometer or thermometer indicated anything of the kind

Mr. Winter called attention to the difficulty of testing from earth currents. He would not go into that now, but he hoped, on a future occasion, to give a paper upon it; it was an important subject, and had occupied a good deal of his attention; in fact, in 1865-6, Sir Wm. Thomson and he were employed by the T. C. and M. Co. to devise a system of testing for the 1866 Atlantic cable. Their system is the one generally used with long cables, and one difficulty they experiencd was to find a satisfactory method of eliminating the earth currents. While the earth currents continue uniform in strength, or vary uniformly, there is no difficulty that cannot be overcome by a little skill, but with long cables, which require considerable time to become uniformly charged, the variation of strength in the earth currents causes great uncertainty as to the true state of the cable.

In determining the locality of a variable fault, the variability of the earth currents is a most serious obstacle, and when the cable is long like the Brest St. Pierre cable, the usual methods of testing entirely fail.

He had devised and practised a method of simultaneous observation at each end, which eliminated the earth currents, although variable, and gave very accurate results.

He could not, without diagrams, explain this mode of procedure, but would do so at some future time.

Another very important thing Mr. Winter had drawn attention to was making one section of the line, in Mance's method, an artificial line; but he would mention that though Mance's was a very pretty system, there was, in his opinion, a serious objection to its use, viz., that every time the resistance was varied to get the true reading, the zero shifted. This shifting of the zero greatly perplexed the observer, and in the case of long cables often made it next to impossible to get any reliable reading at all; it was, however, a convenient mode of working in the laboratory, for getting the resistance of batteries.

The next point Mr. Winter alluded to was the artificial line. It they referred to a patent of his (Mr. Varley's) in 1862, they would see the whole thing fully described.

What was the cause of earth currents? That they abounded most during the time of aurora, was quite clear. What is aurora? He thought there was very little doubt aurora was due to electric discharge from the upper regions of the air, to still higher and more attenuated regions of air. And then, whence came the electricity

of the atmosphere? He thought they would find it was due to the rotation of the earth. They knew that when the atmospheric pressure in a tube was reduced from the present pressure of 30 inches of mercury down to about 1-100th part of an inch, the atmosphere in the tube had a conducting power, roughly speaking, as 500 is to 3. To get to such an attenuated atmosphere they would have to go up to an elevation above the earth of about 40 miles. The earth was a magnet rotating inside a conductor. If they rotated a magnet inside a conductor, they would find the two poles differently electrified to the centre of magnet, as Faraday had shown. If the world be a permanent (or otherwise induced) magnet, and if its equator move from West to South, as is actually the case, the two poles will become plus or positive to the equator.

He (Mr. Varley) believed that at the poles this positive electricity flew off to the upper attenuated regions of the atmosphere which were, comparatively speaking, good conductors, and thus the air became charged positive to the earth.

He believed that this would ultimately be proved to be the true explanation of atmospheric electricity. This, however, must be received with caution at present.

He had devised a series of experiments to establish this hypothesis, but had not as yet been able to finish them. There was much probability at present but no proof that this was the source of terrestrial electric phenomena.

Whether earthquakes gave rise to electric currents or not he did know, but in 1864, while testing the Atlantic cable, his assistant, Mr. Deacon, who was testing 120 miles of the cable in a tank at Greenwich, felt the shock of the earthquake. The galvanometer needle swung out to 90°. On receiving intelligence of that he immediately went to Greenwich Observatory, and saw Mr. Ellis, assistant at the Royal Observatory at Greenwich, who informed him that at the date in question, October 6, 1863, 3.23 a.m., in using the Altazimuth instrument of the Observatory, he was about to make an observation of a certain fixed star, when he saw the star move in the field of view through a small arc, and back again, showing that the same cause had operated to produce the effects that were observed at the Greenwich Observatory and in the cable, the tilting or rocking of the tank was quite sufficient to produce the momentary current.

Mr W. E. Ayrton said, with reference to Mr. Winter's remarks

on the importance of having regular observations at different places, of natural earth currents, I may mention that to a certain extent an attempt has been made to do this in India. Every one of the regular tests of the Government telegraph lines is made with positive and negative currents, and from the different values so obtained, the electromotive force of the natural line current, in terms of that of a single Daniell's cell, is always calculated. This determination of the natural electromotive force is as regularly made as are the conduction and insulation tests of the lines. For simplicity's sake however, it is necessarily assumed that the difference between the positive and negative test readings is due to the potential of the line produced by natural causes, regularly diminishing from one end of the line to the other, or in other words, to a natural current uniform throughout the whole line. This, of course, is frequently not the case in practice, as probably there are instances of natural currents flowing from the earth into the line at or near its centre, and leaving the line at both of its ends, or *vice versa*, the connection between the earth and the line at its centre being caused by dirt or moisture on the insulators. Now such a current would be caused when the potential of the earth, at a badly insulated portion of the line, was greater or less than that of the earth at either of the two ends. When the potentials of one end of a line, the centre, and the other end respectively are A, B, and C, all unequal, and A greater than B, but B less than C, then any calculation of these potentials from the ordinary tests made with positive and negative currents, becomes extremely complicated. What it would be most interesting to know, but what, unfortunately, it is almost impossible to ascertain practically, would be what is the potential due to natural causes of the different points of long line at different times, referred to a fixed potential. Mr. Graves and Mr. Cromwell Varley have spoken of earthquakes being preceded by strong line currents. An example of this I brought before the Asiatic Society of Bengal, in June, 1871, in a paper I read at that time, but as it was, as far as I could then ascertain, a solitary instance, I did not lay very much stress on it. However, as this subject has been mooted here this evening, this additional evidence may be interesting to those present. From tests made partly on the 10th, partly on the 11th, and partly on the 12th of February, 1871, it appeared that strong natural currents were flowing through the telegraph lines in India, in the direction Dacca to Agra, Indore to Agra,

Allahabad to Agra, Agra to Umballa, Calcutta to Raneegunge, and Calcutta to Sahibgunge, in all cases in the same direction, southward to northward, and were strongest where the line ran almost due north and south. This electrical disturbance, apparently universal over the greater portion of India, was followed a day or two after by an earthquake.

Mr. C. F. VARLEY begged further to remark that the earth currents he had chiefly alluded to this evening were those powerful ones which existed whenever the auroral stream was about, or heavy thunder storms were at hand. The delicate apparatus used on long cables were actuated by very small currents, and sometimes a great deal of trouble resulted. When they opened the station at Brest, the earth connection was a copper plate, buried in the town of Brest. It was impossible to work that line. There were continual shocks or kicks which kept the needle in a state of vibration. He greatly reduced them by carrying an insulated wire down into the harbour, and at the end of the pier sinking a large copper plate attached to the wire, to make connection with the sea instead of with the ground.

Great discussion took place about the origin of these currents. He observed them in 1858, and pointed out that the true remedy was to carry the earth connection some distance out to sea. Very often on the French Atlantic Cable indication of thunder and storms were given days before they arrived. They began by little shocks, and increased as the storm came onwards to the coast, and then they were very strong.

Mr. WARREN said, though he would not pretend to enter into the principle of the subject, he thought it desirable, when an important subject like this was introduced, that the paper read should be printed previous to the meeting, as was done on the last occasion, and circulated amongst the members who took an interest in it. With reference to the subject alluded to, they had heard some most interesting observations from Mr. Varley, and there were many points of interest in Mr. Winter's papers; but he thought everyone who had listened to them must go away convinced that this is a subject which is hopelessly in a fog. His own belief was this: they ought not only to know the difference of earth currents produced upon a cable by the different meteorological influences which might visibly produce an effect upon the telegraph, but also the geographical and geological position in which a submarine cable should be laid; and until they

had these solved it was impossible to discuss with scientific accuracy the influences of earth currents; for not only might earth currents be produced by meteorological influences, but they ought to take into account the results which might arise from geological position; and as this Society was starting into existence—he might say under the most flattering auspices—it would be well that they should call upon the different submarine Telegraph Companies to favour them with the results that had been obtained from the observations of earth currents over a considerable period of time. He hoped the council would give this matter their consideration.

The Chairman said it was the intention to have read, this evening, a paper by Professor Fleeming Jenkin, but as the time was so advanced it would have to be postponed till the next meeting. He would, therefore, call upon Mr. Winter for any observations he had to offer in reply upon the discussion.

Mr. Winter said he had listened with great pleasure to the observations of Mr. Varley. His own experiments, to a great extent, confirmed what Mr. Varley had said about the rapid reversal of the current. Mr. Varley had described the method of testing conductivity of the conductor, which he said was used on the cables laid by the Telegraph Construction and Maintenance Company. He (Mr. Winter) had not heard that it had ever been used before, but he dare say most persons would recognise it as being exactly the system he (Mr. Winter) pointed out this evening, the principle of which being gathered from studying the distribution of potential in the bridge, as shewn in Fig. 5, and its application as shewn in Fig. 6. In Mance's system Mr. Varley complained that the zero required to be altered at each adjustment. If the two *proportional* resistances be kept the same, and the resistance of the fourth branch altered to make the adjustment, that would be the case, but Mance had avoided that by keeping the fourth branch constant, and making the adjustment by means of a sliding contact maker, which altered the ratio of the proportional resistances without altering their shunting power. So long as the earth current was constant, the zero, which was independent of the adjustment, remained constant also.

Mr. Varley had called attention to the artificial cable he had patented in 1862, but he (Mr. Winter) was not aware of its application to testing. He did not know it had ever been applied to testing before. With regard to the theory of the rotating magnet, Ampére

had shown that the current generated is generated in the conductor, which does not partake in the rotation. In order to have a current from a moving magnet, they must have some conductor outside which does not move, or there must be relative motion between the conductor and the magnet: in other words, the conductor must pass through the lines of force. If the conductor moved with the magnet they had no current. He did not see in the case of a rotating earth where such a conductor could be found. Of course the winds moved, to a certain extent, the air, but he did not think that was sufficient to account for the immense difference of potential which caused the effect which they called aurora. These shocks which Mr. Varley last spoke of he (Mr. Winter) had noticed himself, and he found that they were remedied by the use of the induction coil instead of the condenser, the cable being connected with the primary, and the receiving instrument with the secondary wire. The magnetic inertia of the iron core apparently absorbed these sudden and instantaneous impulses. He hoped before his return to India to give some further results of his observations on this subject.

The Chairman, in closing the discussion, said he had no doubt some of the members who did not take much interest in scientific subjects, came to the meeting under the impression that there was very little to be said about earth currents, and probably many stayed away, thinking they would not hear anything that would interest them; but those who had listened to the three papers and the discussion upon them, must have felt themselves amply repaid for their presence. They had had three interesting and valuable communications. The first, from Mr. Stout, was one for which they felt much indebted, and for which he was sure the meeting would accord him its thanks. That gentleman made a suggestion which he (the Chairman) made himself some years ago in print, viz., that aurora might be caused by the induction of flashes of auroral light, and that the earth currents which appeared to be chasing each other in all directions during the existence of aurora, might be due to positive and negative currents of electricity passing over the earth, and driving away, by induction, a current which afterwards returned to its place. Such an idea was confirmed when they looked at the diagrams to be seen at the Royal Observatory of the force and direction of the currents. It would be found they had a tendency to keep varying capriciously from maximum to minimum, and they knew there must be a cause for that

change, and the suggestion might guide them in endeavouring to discover what the cause of these fluctuations was. He considered that the currents were derived from the earth, and not from the air. He (the Chairman) had no doubt their wires did receive currents from the atmosphere, but not currents of such tension, or in such available quantity as to have any effect upon the instruments. Mr. Stout had remarked, that on some occasions the needles were deflected from right to left, and *vice versa* at neighbouring stations, as if the current entered the wires at the centre of their length, and not at the end. That would be interesting, if true, but it needed further confirmation before it was received.

Upon the paper of Mr. Winter he need bestow no commendation, because they all appreciated it on its own account. Mr. Winter alluded to the magnetic forces of the earth, and observed that the variations of intensity of the horizontal forces were due to the variation of the currents of electricity passing through the earth. He thought the author of the paper was right in this; and it was an idea which he (the Chairman) had long had in his mind. It was probable that the sun exerted an enormous power over the currents of the earth. They knew that during each eleven years the sun's spots increased and decreased, and during that period the aurora became proportionately more or less intense. They knew there were rainfalls varying in the same manner, and it was clear that our magnetic force was dependent upon the sun. In 1850 two simultaneous observations of the sun were made by observers many miles apart, when both saw a body flash into the sun and cause a disturbance of the sun's chromosphere; and it was subsequently that at that moment almost all the magnets in the world were disturbed by this sudden movement. At first sight it seemed curious that the effect should have been instantaneous. One would have imagined that some time might have elapsed before the effect was apparent upon the earth's surface; but it went to show that the velocity of the transmission of magnetism was the same as the velocity of the transmission of light, and if so, it was natural that at the moment the phenomenon was apparent, the effects were apparent on the instruments in the neighbouring observatories.

Mr. Graves had given some interesting observations, and, amongst other things, had shown that there are diurnal maxima and minima of earth currents, and that the maximum occurred about noon.

He also made observations indicating some connection between earthquakes and earth currents. That had been confirmed by Mr. Varley, and was interesting if it turned out true. It was to be hoped that all the members would bear this subject in mind, and make observations, and communicate them to the Society.

Mr. Varley had not read a paper, but his observations were not less interesting on that account. He spoke of the non-conductivity of the earth at Lynn and Ipswich as a reason why the current was observed on the wires: but there was no trace of currents passing through the sea. They did not get violent currents in submarine cables. He thought the sea was a good conductor, which prevented the great change of potential which occurs on the earth. Mr. Varley also spoke of the amount and force of the current developed in the wires. He (the Chairman) could say this potential was sometimes so high that it produced sparks at the terminals of the wires, and Mr. Varley said at one time it led him to fear for the longevity of our cables. He (the Chairman) had had the same idea, but he was very glad to find that the cables had stood so many years without receiving serious damage from earth currents, though no doubt when cables became partially injured they suffered from earth currents. He had himself studied the question, whether it was best to retain the ends of the cable insulated during storms or connect them with earth, and his opinion was they were safer when connected with the earth; because if they had a tension of 2,000 or 3,000 cells at one end of a cable it was a great matter to relieve the other end of such a strain.

Before sitting down he would make a remark on Mr. Varley's theory of the rotation of the earth being the cause of electric currents. It was one more proof of how the same idea often occurred, simultaneously, to many minds. It was a subject which he had himself been considering for the last few weeks, whether it might not be true that the rotation of the earth was the cause of the electric currents which pass through the earth. Mr. Winter objected that there was no part of the earth or of the atmosphere which was not moving; and, therefore, there could not be such a phenomenon as the relation of a magnet and a fixed conductor; but he thought, by comparison, the higher regions of the atmosphere might be relatively stationary in respect of the rotation of the earth itself, and so become a conductor sufficient to produce electric currents.

In reply to what had fallen from Mr. Warren and his suggestion

that the papers should be circulated before they were read, in order that members might come there prepared to discuss them, he would say he believed that system was adopted by the Institution of Civil Engineers generally, though not invariably. He was happy to say the Council had been discussing that question for the last two or three meetings, and they intended to commence this plan by printing the paper which was to be read by Mr. Fleeming Jenkin at the next meeting; but he could not undertake to say that practice would be followed, as it was yet under consideration. In conclusion, he would say he entirely endorsed the suggestion of Mr. Winter, that members in their several localities should make practical observations into these scientific matters, and communicate the results to the Society. It now only remained for him to propose a vote of thanks to the gentlemen who had favoured them with the communications that had been read this evening.

The following Candidates were balloted for and declared duly elected:—

As a Foreign Member:—
J. Georg Repsold, Rio Janeiro, Brazil.

As a Member:—
Mortimer Evans, West Regent Street, Glasgow.

As Associates:—
Edward Adamson, Submarine Telegraph Company, Eastbourne.
Thomas Ascough, Post Office Telegraphs, Cannon Street.
Henry Ben'est, India Rubber Company, Silvertown.
Commander Fisher, R.N., H.M.S. "Excellent," Portsmouth.
Douglas Pitt Gamble, 26, Coleberne Road, South Kensington.
Walter Hancock, 10, Upper Chadwell Street, Myddleton Square.
James Laister, Post Office Telegraphs, Cannon Street.
G. H. Riddle, Telegraph Account Branch, General Post Office.
Alfred Sheath, Auckland, New Zealand.
Henry Tubb, India Government Telegraph, Indore, Central India.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

AUTOMATIC TELEGRAPHY.

In the First Number of our Proceedings, at Page 51, reporting the discussion upon Mr. Culley's paper on "Automatic Telegraphs," I mentioned that I believed that a system of Automatic Hughes working had been tried in Italy. I find I was mistaken. I learn from the Director General of Italian Telegraphs, M. d'Amico, that it was Bonelli's "Typo-telegraphe" which was thus tried.

W. H. PREECE.

NORTH-WESTERN TELEGRAPH CO.

General Superintendent's Office,
Milwaukee, Wis.,
March 12*th*, 1873.

Geo. E. Preece, Esq.,
Sec. S.T.E. London.

Dear Sir,

Mr. Geo. B. Prescott, Electrician of the Western Union Telegraph Co., took to London with him some samples of the "Kenosha Insulator," made by the "Kenosha Insulator Co."

We are using them upon our lines, having put up, last year, over 60,000. We shall use more this year. The Western Union Co. has adopted the Kenosha, and will use it hereafter.

Mr. F. L. Pope, of New York, has a record of tests made by Mr. C. H. Summers and me, and he will forward it to you, with the result of his own tests, in a short time, embodying them in a paper to be read before the Society.

Some of them, rudely made, five years ago, are still upon the wires to which they were first attached, and the material shows *no sign of deterioration.* All the gaseous or volatile products of the gas tar being eliminated, the tar combines with the pulverized charcoal perfectly, making a compound that is elastic, adherent to the wood, and shows no change under any temperature.

On my lines in Northern Minnesota and British America, they have passed through the severest winter ever known, without change.

If you wish some of them to test, and have any choice in form, we will gladly follow any sketch you may send us.

Under the most favourable circumstances for glass, the "Kenosha" has never tested less than *five times* higher than the glass—while with an atmosphere showing 90 per cent. of saturation, the "Kenosha" shows from 8 to 12 times in excess of glass.

Comparative tests made with the Varley, and various forms of European porcelain, show the Kenosha to be far ahead, uniformly, under precisely similar conditions.

Under these circumstances, we feel warranted in calling the attention of the Society to them—feeling that we have an insulator that can be furnished cheaply, and one also that will, for insulating properties and durability, exceed anything heretofore manufactured.

Should there be any information desired, relative to American instruments, batteries, or method of working circuits, I will be happy to give it.

I should like information on one subject, *i.e.*, "repeaters," or, as you term them, "translators," for repeating from one circuit to another. I have seen sketches only of those given in "Culley." They are in extensive use in this country, enabling San Francisco on the Pacific to work with New York, over 3,000 miles. As a rule, we work single circuits of 500 miles, except in very bad weather; beyond that we use repeaters. On the lines of this Company we work from Chicago, Illinois, to St. Paul, Minnesota, in one circuit, 490 miles, and through a repeater there, with Fort Garry, Manitoba (Western British America), 620 miles, making a total of 1,110 miles.

You have probably seen, in "Pope's Practice," the Hicks and Bunnell repeaters. In addition to those are mine (three forms) Facer's, Gray's and one or two others.

Respectfully,

C. H. HASKINS.

THE ACTION OF OAK UPON EARTH-WIRES.

By James Sivewright, M.A.

A short time since I received instructions to paint and earth-wire a large consignment of oak arms. As they were completed they were stacked and issued as occasion demanded. The earth-wires, which consisted of the ordinary No. 16 galvanised iron wire, shortly after

being fixed showed signs of corrosion; the action increased day by day, and a white powder in time covered their entire surface. Complaints came in from every foreman to whom the arms were sent: "the earth-wires," it was alleged, "had no substance in them; they broke off in the workmen's hands, and were, in fact, worse than useless."

The cause of this was at first attributed to the place in which they were stored. It was damp, underground, and the sulphurous gases would, it was thought, have the effect which was observed. The arms were accordingly moved to another story of the building, which was drier and admitted of a freer circulation of air; but no improvement resulted in the condition of the earth-wires, the corrosive action still going on as before.

It was then suggested that the paint which was being employed might be to blame. Accordingly two plain and two painted deal arms were earth-wired and placed alongside of two plain and two painted oak arms, which were also earth-wired, and had been selected from those which were being supplied. After some time they were examined: the earth-wires on the deal arms were not affected in any way; on the other hand, a film of white powder had already covered those on the oak. The cause, therefore, be what it may, was wholly independent alike of the building and the paint.

To set the point at rest a qualitative test was made of the salt scraped from the earth-wires. It consisted of a mixture of acetates, carbonates, and oxides, all clearly marked and well defined. The real agent which all the while had been at work was the natural acid of the oak. It first attacked the zinc and iron of the wire; the carbonic acid present in the atmosphere then stepped in, and in the meantime the process of oxidation had also been going on. The action was perfectly analogous to that which occasionally takes place on the roofs of churches and other buildings, where large masses of imperfectly-seasoned oak and lead are brought into contact. The acetic acid of the oak attacks the lead, forming acetate of lead; thence it is gradually expelled by the carbonic acid in the air, and when thus set free attacks another portion of the lead, which in course of time becomes wholly converted into carbonate of lead—that is, the ordinary *white lead* of commerce. The old process, known as the Dutch method, of preparing white lead is actually there repeated: the oak takes the place of the earthen vessel containing the vinegar (acetic

acid), and the carbonic acid is drawn from the atmosphere instead of being supplied from putrefying organic matter.

An interesting point worthy of remark was further observed in connection with the oak arms. In the immediate vicinity of the earth wires a black discoloration was very plainly to be seen. This was due to the ink which had been formed there, and which, as is well known, is largely produced by the action of the acid extracted from the gall-nuts of the oak tree upon the ferric compounds.

Southampton, *July 5th*, 1873.

NAUTICAL TELEGRAPHY.

By Captain P. H. Colomb, R.N.

A Bill presented by the Board of Trade, now before Parliament, contains the following clauses and schedules relating to Signals:—

18. The signals specified in the first schedule to this Act shall be deemed to be signals of distress. Any Master of a vessel who uses or displays, or causes or permits any person under his authority to use or display, any of the said signals, except in the case of a vessel being in distress, shall be liable to pay compensation for any labour undertaken, risk incurred, or loss sustained in, consequence of such signal having been supposed to be a signal of distress and such compensation may be recovered in the same manner in which Salvage is recoverable.

19. If a vessel requires the services of a Pilot, the signals to be used and displayed shall be those specified in the second schedule to this Act. Any Master of a vessel who uses or displays, or causes or permits any person under his authority to use or display, any of the said signals for any other purpose than that of summoning a Pilot, or uses, or causes or permits any person under his authority to use any, other signal for a Pilot, shall incur a penalty not exceeding £20.

SCHEDULE I.

Signals of Distress.—In the daytime: The following signals, numbered 1, 2, and 3, when used or displayed together or separately, shall be deemed to be signals of distress in the daytime:—1. A gun fired at intervals of about a minute. 2. The *International Code* signal of distress indicated by N C. 3. The distant signals, consisting of a square flag having either above or below it a ball, or anything resembling a ball. At night: The following signals, numbered 1, 2, and 3, when used or displayed together or separately, shall be deemed to be signals of distress at night:—1. A gun fired at intervals of about a minute. 2. Flames on the ship (as from a burning tar barrel, oil barrel, &c). 3. Rockets or shells, of any colour or description, fired one at a time, at short intervals.

SCHEDULE II.

SIGNALS TO BE MADE BY SHIPS WANTING A PILOT.—In the daytime: The following signals, numbered 1, and 2, when used or displayed together or separately, shall be deemed to be signals for a Pilot in the daytime viz.:—1. To be hoisted at the fore, the jack, or other national colour usually worn by Merchant ships, having round it a white border, one-fifth of the breadth of the flag; or 2. The *International Code* Pilotage signal indicated by P T. At night: The following signals, numbered 1 and 2, when used or displayed together or separately, shall be deemed to be signals for a Pilot at night viz.:—1. A blue light every 15 minutes; or 2, A bright white light, flashed or shown at short or frequent intervals just above the bulwarks, for about a minute at a time.

The *Shipping and Mercantile Gazette* of May 20th, contains the following very apposite remarks on these clauses:—

The Board of Trade, as may have been seen by the Bill to "Amend the Merchant Shipping Acts," proposes to settle the question relating to signals of distress at night by 1st—A gun fired at intervals of about a minute; 2nd—Flames on the ship (as from a burning tar barrel, oil barrel, &c.); 3rd, Rockets or shells of any colour or description, fired one at time at short intervals. These three methods may be used together or displayed separately. In November last we published a copy of a letter from the Local Marine Board at Newcastle, which had been sent to the Board of Trade in reply to a circular, on the subject of legalising distress signals. The Local Marine Board protested against the intended plan of enforcing signals of distress, on the ground that the Shipowners and Underwriters of the Tyne district deprecated any interference with the system prevailing. "Sailors of all nations [said the Secretary of the Board] "know distress signals whenever and wherever the same are exhibited, and "distress signals are easily understood when seen by persons on the sea coast. "It is extremely undesirable to hamper persons in distress with exhibiting "strictly legal signals. It is only interested parties on shore that can desire the "signals to be legalised." The Local Marine Board were right as a general rule, but such a case as that of the *Northfleet* has proved that there may be exceptions. Will, therefore, the signals now submitted to the House of Commons tend to lessen the sacrifice of life at sea by making the position of vessels more readily ascertainable? We should say that the attempt would end unfavourably. Supposing that guns, flames, and rockets could be exhibited or used together, the result would be effective for the object; but the scheme could not work practically, and for the following reasons:—1st. as regards guns. Masters of ships arriving off the coast at night fire guns to attract the attention of Pilots. Should this Bill pass they must no longer do so, or even throw up a rocket. If they do, they will be liable to make good, by way of compensation, for any labour undertaken and risk incurred. Mail packets and steamers would be seriously disadvantaged by this scheme, and rather than be kept waiting off the coast in near proximity to danger, to the peril of passengers and crew, their Masters would disregard the law, and this would soon end in rendering distress signals by gun almost a nullity. Next suppose as in the case of the *Northfleet*,

the gun cannot be fired, or there is no gun on board to fire, or the powder is wet, that means of making the signal would be non-effective. 2nd. As to flames. This signal may be made by itself, and is to be a token of distress. Art. 9 of the "Regulations for Preventing Accidents at Sea" enacts that fishing vessels and open boats shall not be prevented from using a flare-up. Under the American Law, every sailing vessel when approaching a steam vessel is to display a torch light. These flare-ups and torch lights will be confounded with distress signals. 3d. Rockets or shells of any colour or description, fired one at a time, are to be signals either by themselves or together with guns and flames. It happens too frequently that, when a ship is in distress, and the men have to take to the rigging, that the rocketbox is washed over board, or cannot be got at. In many instances the rockets could not be fired from the deck of a ship, even if they were accessible. The rocket, however, would be a valuable adjunct to signalling if adapted to colours. The indiscriminate use of rockets would destroy their value as danger signals; besides which our Government has no means of stopping their use by foreigners off our coasts. Pyrotechnic lights are exceedingly common, and, among many other lines of sailing ships and steamers, are used by the following Transatlantic lines:—

CUNARD LINE.—Two rockets and one blue light.

INMAN LINE.—Blue light forward, red light amidships, blue light aft, and two rockets. The lights all to be burning at the same time.

GUION LINE.—Blue light forward, blue light amidships, and blue light aft—all burning together.

NATIONAL LINE.—Blue light, one rocket, and a red light.

ANCHOR LINE.—Red and white light alternately.

MONTREAL MAIL COMPANY.—Blue, white, and red rocket, in succession.

FRENCH LINE.—Blue light forward, white light amidships, and red light aft, exhibited simultaneously.

NORTH GERMAN LLOYD'S.—Blue light forward, blue light aft, two rockets.

HAMBURG—AMERICAN LINE.—One roman candle, one rocket, and one roman candle, with an interval of a minute between each.

NEW YORK AND LONDON LINE.—Rocket, blue light, rocket.

ROGERS' LINE.—Blue light and red light amidships, and both burning at the same time.

None of these ships would be allowed to signal at night at sea, or near the coast, by rockets or blue lights; but the chances are that they would be compelled to employ such signals in defiance of any law that might be passed. A steamer with mails and some hundreds of passengers must not be imperilled simply because a law may be enacted forbidding the use of guns, rockets, and blue lights. In fact, the exclusive use of these things except as distress signals, would be a matter of impossibility. This kind of panic legislation will defeat the object in view. If the rockets or blue lights alternated with each other, and made some express signal, the system might be intelligible; or if rockets only were displayed, following each other in different colours, and detonating in the air, their use might be found serviceable. To declare, however, that no Shipmaster shall fire a gun at sea or cause a rocket to be displayed,

seems too preposterous for belief. As we have said upon a former occasion, when a ship is in distress her crew must do the best they can to make their position known. It too often happens that at such a time the seas are sweeping the decks, and therefore, a standing signal of distress aloft is wanted in addition to guns, rockets, flames, shells, and other methods. The grand end is to direct attention to the spot, and, when the vessel in distress is seen, to understand that assistance is wanted. This winter a ship was fourteen hours on the Goodwins before her signals were made out. Passing ships might possibly have made known her condition earlier, or rendered assistance, if a fixed distress night signal of a defined order could have been displayed at any of the halyards. Fire, noise of guns, and rockets are excellent in their way to attract attention; but beyond all these a masthead or yardarm signal is wanted as a complement to gunpowder and flames. Such a supplementary fixed signal it might be possible in the majority of cases, to hoist, and, where it could not, the other adjuncts would, perhaps, be unserviceable and inapplicable, in consequence of the wind, the weather, and the seas.

In only one point does the *Shipping Gazette* appear to be misinformed, that is on the possibility of establishing a *permanent* signal at night on board ships. There are few matters in Nautical Telegraphy which have been subject to larger or more exhaustive experiments than permanent night signals. It may be enough to say that they had been in use for 200 years in the English Navy when the flashing system, with the Morse dot and dash, superseded them as it has done, completely.

ON THE INTERNAL RESISTANCE OF BATTERIES.

In measuring the internal resistance of any number of cells at once by double deflections, and calculating that resistance by the well-known formula

$$x = \frac{Rd - Dr}{D - d}$$

the slightest variation in the value of the divisions at different parts of the mirror scale introduces an element of error which will increase the more as the pairs of deflections (D and d) are taken wider apart.

Finding that by working with deflections at different parts of the scale I could not get the results to agree with each other, I turned my attention to the determination of the exact value of each hundred divisions throughout the scale.

The galvanometer used in these experiments was astatic, having one coil over the other, the mirror being in the upper coil, and a fan in the lower one, the mirror and fan attached to magnets opposed to each other, and a controlling magnet sliding vertically on a rod, and

capable of adjustment either vertically or horizontally. This galvanometer was made by Messrs. Elliott Brothers, London (No. 156), and has a resistance of 5854 ohms.

Its constant was so adjusted by the controlling magnet that 50 cells through a sum of all resistances in circuit equal to 100,000 ohms, using the 1000th shunt, produced a deflection of 100 divisions from zero; or, a constant of 10^{10}.

Having found this to give a continually steady deflection of 100, the spot was turned 350 divisions to the right of zero and the deflection noted upon opening the galvanometer. Shifting the spot consecutively 50 divisions farther to the left throughout the entire scale, and noting the deflection obtained each time, it gave the results shown in the following table.

The constant was checked three times during the operation. The readings were repeated the next day with precisely similar results.

From numerous trials over various portions of the scale, the electromotive force in each case being the same, the following comparative values were obtained, the ranges varying—

If, therefore, the range be taken between

50	right and	50	left, the true value is	101
100	„	100	„	202
150	„	150	„	303
200	„	200	„	406
250	„	250	„	510
300	„	300	„	614
350	„	350	„	721

To avoid error as much as possible, I took a few readings for the resistance of 50 cells at nearly the same deflections with the following results—spot turned to 350 divisions to the right of zero, and deflections obtained towards the left:—

No.	d	R	D	r	x by formula
1	100	102,000	111	97,000	9,000
2	200	52,300	210	49,500	6.500
3	400	24,480	410	23,800	3,400
4	600	15,460	610	15,160	2,840
5	700	12,960	710	12,760	1,240

The deflections in each pair being so near each other the variation due to inequalities of scale cannot very materially affect the results, yet it will at once be seen that their range is very great.

I then resolved to test the accuracy of these results by comparison with Sir William Thomson's formula, using the same 50 cells, as follows:—

a and b being equal deflections.

If $s = 1$ ohm, the formula is $\frac{R}{g} = x$.

If $s =$ more than 1 ohm, then $\frac{Rs}{g} = x$.

But if the galvanometer be too sensitive for the latter arrangement with a larger shunt than 2 or 3 ohms, by inserting resistance at r in *Fig.* 1, to obtain a, the formula becomes $\frac{Rs}{g} - r = x$ when r is very small as compared with g; but, more accurately, the formula becomes $\frac{Rs}{g} - \left[\left(\frac{g+s}{g}\right) r\right] = x$.

Having reduced the galvanometer to its least sensitive state by lowering the controlling magnet to the bottom of the vertical rod, I obtained the following results:—

Shunt ohms s.	Galvr. resist. g.	Resist. at r Fig. 1. r.	Deflect. a & b	In Fig. 2. R	× S	Total R in Fig 2.	Value of x 50 cells.	x per cell.
1	5,854	nil	158	12,000	1,000	12,000,000	2,050	41.0
1	do.	500	132	15,000	1,000	15,000,000	2,062	41.2
1	do.	1.000	114	17,760	1,000	17,760,000	2,034	40.6
2	do.	nil	305	5,100	1,000	5,100,000	1,742	34.8
2	do.	500	254	6,600	1,000	6,600,000	1,754	35.1
2	do.	1,000	219	8,200	1,000	8,200,000	1,801	36.0
3	do.	nil (Zero 200 right.)	476	2,360	1,000	2,360,000	1,209	24.2
3	do.	500	394	3,380	1,000	3,380,000	1,232	24.6
3	do.	1,000	338	4,400	1.000	4,400,000	1,254	25.1

These results show that the three variations of the formula $\frac{R}{g}$, $\frac{Rs}{g}$ and $\frac{Rs}{g} - r$ give practically the same result, *provided always* that the shunt remains constant; but the moment the shunt is altered, the internal resistance also shows an alteration.

These results were taken not long after the battery was set up. A month afterwards, when this battery had worked up to good con-

dition, giving only about half its former resistance, I took a further series of readings in confirmation of the foregoing ones, as follows:—

Shunt ohms s.	Galvr. resist. g.	Resist. at r in Fig. 1. r.	Deflects. a & b.	In Fig. 2.			Value of x 50 cells.	x per cell.
				R	× S	= Total R.		
1	5,854	nil.	355	5,600	1,000	5,600,000	956	19.13
1	do.	500	243	8,640	1,000	8,640,000	975	19.51
1	do.	1,000	188	11,500	1,000	11,500,000	964	19.29
2	do.	nil.	715	2,240	1,000	2,240,000	765	15.30
2	do.	500	502	3,700	1,000	3,700,000	764	15.28
2	do.	1,000	387	5,200	1,000	5,200,000	776	15.53
3	do.	nil.	1,080	1,100	1,000	1,100,000	563	11.27
3	do.	500	750	2,100	1,000	2,100,000	576	11.52
3	do.	1,000	577	3,060	1,000	3,060,000	568	11.36

These results, whilst they show a great reduction in the resistance of the battery during the month, show also that the same effect is observed by the change of shunt, as in the former series.

Previous to taking the above series I took a hasty preliminary test with four different shunts with the following results, which do not differ much from the more deliberate and accurate tests tabulated above.

With a shunt of 1 ohm the result for 50 cells was 939
„ 2 ohms „ „ „ 758
„ 3 ohms „ „ „ 574
„ 4 ohms „ „ „ 372

This appears to indicate that a still further reduction in the resistance of the battery might be made by further increasing the value of the shunt employed, the resistance per cell in the above series being 18·8, 15·1, 11·5 and 7·4 ohms respectively.

These results show plainly that the *higher* the deflection and the *less* the resistance in the circuit the *less* is the internal resistance of the battery; and, on the contrary, the *lower* the deflection, and the *higher* the resistance in the circuit, the *greater* is the internal resistance. The *more external work* there is to be done the *more* is the resistance of the battery itself internally.

The battery consisted of a copper plate at the bottom of a jar, sulphate of copper over it, wet sawdust over the sulphate of copper, and a heavy disc of zinc on the top of the sawdust.

I have tried to trace the cause of this peculiarity, and I can only come to the conclusion, that as high as the resistance is in the circuit

so high in proportion is the tenison of the battery to perform the work. As this work increases so must the chemical action in the battery increase to provide for its accomplishment. This extra chemical action decomposes the moisture in the battery more rapidly than with a lower resistance and tension, and forms the gases proportionally quicker.

To this cause I am led to attribute the increase in the internal resistance of the battery upon an increase of resistance being placed in its circuit.

This should teach us, therefore, that it is not safe to take the internal resistance of a large number of cells at once, and having come to this conclusion, at the time the *first series* in this paper was taken, I took two of the cells and measured their resistance as follows by the formula $\frac{Rs}{g} = x.$ $g = 5{,}854$ ohms.

Shunt s	Deflection.	Resistance R	x 2 cells.	x per cell.
ohms 1	69	141,200	24.1	12.05
2	129	77,000	26.3	13.15
3	202	49,000	25.1	12.55
4	271	36,320	24.8	12,40
5	339	29,000	24.7	12.35

mean = 12.50

These results, with variable shunts, are far more alike than any other series with a larger number of cells.

It seems, therefore, to be the right method to take but 1 or 2 cells from the large battery and measure by proportion the comparative resistance of the whole. Far less error is thus likely to creep into the results than by taking the resistance of the whole battery by one operation.

JAMES GRAVES.

Valentia, *March* 15*th*, 1873.

ON COILING OF SUBMARINE TELEGRAPH CABLES.

THE quantity of Cable which can be stowed in a Tank with a cone in the Centre depends upon the following measures:—

Diameter of Tank (feet) - - = D
Mean Diameter of Cone (feet) - = d
Height of Tank (feet) - - - = H
Diameter of Cable (feet) - - = k

Length of Cable in the middle flake is found in the following way:—

Length of middle turn - - - $= \frac{D + d}{2} \pi$

Number of turns in middle flake - $= \frac{D - d}{2\, k}$

therefore, length of Cable in middle flake $= \frac{\pi}{4} \cdot \frac{D^2 - d^2}{k}$

Number of flakes in height of tank is determined by $C \cdot \frac{H}{k}$, in which C is a co-efficient, depending on the closeness and care with which the Cable is coiled, the packing, &c.; consequently, total amount of Cable, in feet, is equal to—

$$L = C \cdot \frac{\pi}{4} \cdot \frac{H\,(D^2 - d^2)}{k^2}$$

Now, if the coiling be carried out by experienced workmen, the value of C will be found to remain the same, and constant for cables of different sizes. From actual coiling on board cable ships, the writer has found the co-efficient $C \cdot \frac{\pi}{4}$ to be equal to 0.75. If now $H = 1$, then we arrive at the following formulæ for the quantity of Cable which can be coiled in *one foot height* of a given tank, viz:—

$$L \text{ (feet)} = 0.75\,(D^2 - d^2) \cdot \frac{1}{k^2} \quad \text{- - (1).}$$

$$\text{(yards)} = 0.25\,(D^2 - d^2) \cdot \frac{1}{k^2} \quad \text{- - (2)}$$

$$\text{and (naut. miles)} = \frac{D^2 - d^2)}{8116} \cdot \frac{1}{k^2} \quad \text{- - - - (3)}$$

For the sake of convenience the writer has tabulated the first member of equation (3), $\left(\frac{D^2 - d^2}{8116}\right)$, and in the annexed table will be found the moduli for tanks from 25 to 32 feet in diameter, and with cones from 4 to 10 feet mean diameter. To find the length of Cable, in nautical miles, capable of being coiled in a tank of known dimentions, multiply the modulus of the tank by the co-efficient $\frac{1}{k^2}$; k^2 being the square of the diameter of Cable in feet.

Example:—A 2-inch Cable is to be coiled 12½ feet high in a tank of 32 feet diameter with a cone $\frac{6' \text{ base}}{4' \text{ top}}$, required the mileage? Answer: 2″ equal to 0.167′; modulus of tank, $32' \times 5' = 0.123$; therefore, mileage one foot high $= 0.123 \cdot \frac{1}{0.167^2} = 4.40$ naut. miles

and by $12\frac{1}{2}$ feet height = 55.0 naut. miles; 30 naut. miles of this Cable would require a tank of height = $\frac{30}{1.4}$ = 6.8′ + space above coil.

Another Example:—160 naut. miles of a Cable, diameter = 0.071′, are to be coiled $8\frac{1}{2}$ feet high in the tank, with a cone $\frac{5'\,\text{base}}{3'\,\text{top}}$. Diameter of tank required?

In the height of one foot must be coiled $^{160}/_{8\frac{1}{2}}$ = 18.8 n. m., co-efficient $\frac{1}{k^2}$ = 198.4 and therefore

$$\text{modulus} \times 198.4 = 18.8$$

from which

$$\text{modulus} = \frac{18.8}{198.4} = 0.0947.$$

from the column 4′ the modulus thus found gives the required diameter of tank = 28 feet.

When Cables of different sizes are to be coiled in the same ship, it will be found very convenient to reduce the different cables to one and the same standard in the reverse proportion of the square of their outside diameters.

If, for instance, four Cables	A	B	C	D
are of the corresponding diameters	0.071′	0.088′	0.119′	0.180′
then the co-efficients $\frac{1}{k_2}$ are respectively	198.4	129.1	70.6	30.9
and the mileage of Cables in proportion to Cable A will be	100	65.1	35.6	15.6
If now successively B, C and D are taken as standards, the proportion will be found to be, for B = 100...	153.7	100	54.7	23.9
for C = 100...	281.0	182.9	100	43.8
and for D = 100...	642.1	417.8	228.5	100

that is to say, 100 miles of Cable C require the same tank room as 281 miles of A, 182.9 of B, and 43.8 of D.

Suppose, for example, it were proposed to coil in a tank, $26\frac{1}{2}$ft. by 10′, the following lengths of Cables, viz., $1\frac{1}{2}$ miles of C, 30 B, 12 A and 12 D; what will be the height of the whole in the tank?

$1\frac{1}{2}$ C	require	the same as	4.21	A
30 B	,,	,,	46.11	A
12 A	,,	,,	12.00	A, and
12 D	,,	,,	77.05	A

and total amount of standard Cable A = 139.4 miles. Modulus to tank $26\frac{1}{2}$′ by 10′ is = 0.0743, which multiplied by 198.4 gives 14.7 miles of

standard Cable A in a one foot coil, consequently the 139.4 miles A, or the above 55½ miles of different Cables will make a coil of the height $^{139.4}/_{14.7} = 9\frac{1}{2}$ feet.

Modulus for Cable Tanks $= \dfrac{D^2 - d^2}{8116}$.

Diameter of Tank = D.	Mean Diameter of Cone d = 4′	4½′	5′	5½′	6′	6½′	7′	7½′	8′	8½′	9′	9½′	10′
25′	0.0745	0.0745	0.0739	0.0733	0.0726	0.0718	0.0710	0.0701	0.0691	0.0681	0.0670	0.0659	0.0647
26′	813	808	802	796	786	780	773	764	754	744	733	722	710
27′	878	873	867	861	854	846	838	829	819	809	789	787	775
28′	946	941	935	929	922	914	906	897	887	877	866	855	843
29′	1016	1011	1005	999	992	984	976	967	957	947	936	925	913
30′	1089	1084	1078	1072	1065	1057	1049	1040	1030	1020	1009	998	986
31′	1164	1159	1153	1147	1140	1132	1124	1115	1105	1095	1084	1073	1061
32′	1241	1236	1230	1224	1217	1209	1201	1192	1182	1172	1162	1151	1138

C. L. M.

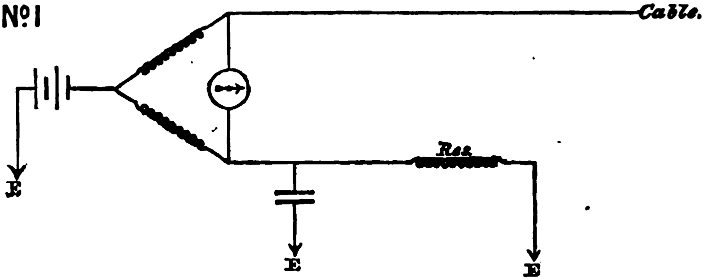
DUPLEX SYSTEM OF TELEGRAPHY ON SUBMARINE CABLES.

By C. V. de Sauty, M.S.T.E., F.R.A.S., Electrician, Eastern Telegraph Company, Gibraltar.

The interest recently shown with regard to the Duplex system of working on land lines, and the general success it has met with telegraphically, has induced me to place before the Members of the Society some notes relative to the experiments carried on through the Gibraltar-Lisbon Cable, a length of 360 knots (417 statute miles), which have resulted in complete success.

Mr. W. H. Preece, M.S.T.E., Divisional Engineer of the Postal Telegraph Department, when passing through Gibraltar recently, on his way eastward, recommended me to try to work the Duplex system on one of the cables terminating at Gibraltar, and gave me the following sketch of connections:—

No. 1



In the year 1855, in conjunction with Mr. Bouffleb and another German Engineer whose name I now forget, I had worked successfully the Duplex, or (as it was then called) the "double-speaking" system of Frischen-Siemens, on a short line between Manchester and Altrincham.

This system depends upon the use of a relay, wound with two separate wires, one of which is joined to the line wire, the other is connected to a resistance equal to that of the line and then to earth. Upon sending, the currents circulate round the electro-magnet of the relay in opposite directions producing no magnetism; but on the distant station sending, the whole of the home station's currents pass through in one direction, and produce signals on both instruments during the time that both keys are down upon the contact anvils.

We found in practice that, although capable of doing on an average, when working singly (or simple Morse), 16 words a minute, when switched over to work the Instruments Duplex, we could not work faster than 5 words a minute from each end: total 10 words—loss 6 words; and I imagine, that it is owing to that fact, that these instruments which had been fitted up in a great many Prussian stations were replaced by the ordinary Morse.

There is one peculiar arrangement of this system that I have never seen in print, and I think is not generally known, and that is, that the instruments at A station can be so arranged by switches that they will repeat back messages to B station without any human intervention. By suitable switches, the lever of the Morse Recorder takes the place of the manipulating key.

On returning from the Crimea in the latter part of 1855 and beginning of 1856, attempts were made to work between London and Birmingham over the wires of the Magnetic Telegraph Company, but Mr. Gordon, of the firm of R. S. Newall & Co.—to whom the English patents belonged—objecting to the expense of constructing large condensers (although a slight improvement in the signals had resulted from the use of a condenser belonging to an induction coil), the experiments ceased without any success.

The system (Frischen-Siemens) has been excellently described by Mr. Robert Sabine, M.S.T.E., to whose work I beg to refer any one desiring further information.

I think that in describing the experiments between Gibraltar and Lisbon, it may be as well to include those that failed, as they may be interesting to the Members of this Society, and may prove useful to those who may repeat them or endeavour to work the system on other cables.

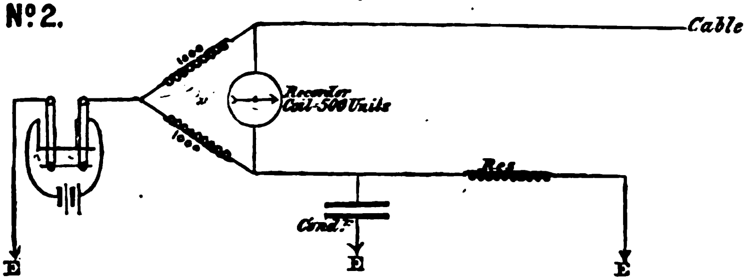
The average resistance of the conductor between Gibraltar and Lisbon during the time of these experiments was 3985 ohms.

The inductive capacity, according to Latimer Clark's Tables, in Clark and Sabine's "Electrical Tables and Formulæ" is 0·348 microfarads per knot. Our original experiments were based upon this estimate, but failing to give good results, we then, by discharge test, determined correctly the inductive capacity, using as a standard one of Warden's Condensers of 20 microfarads—and found it to be 0·217 per knot. Tests were made also at Lisbon, the result there, being somewhat lower than mine.

The insulation of the cable averaged 2170 megohms per knot during the period of our experiments. Its length was 360 knots.

The first experiment, on March 16th, was as follows:—

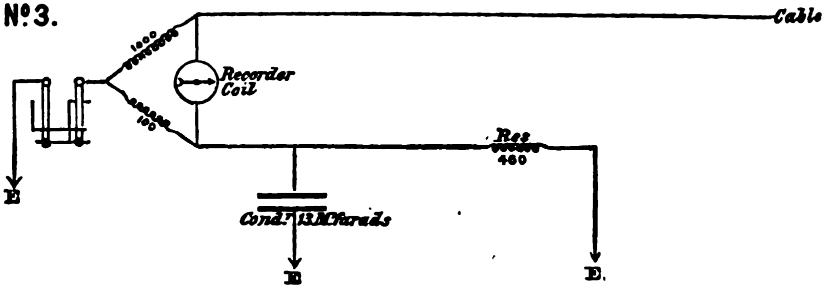
No. 2.



4200 Condenser varying from 7 to 40 microfarads, Lisbon's arrangement being exactly similar. The result of this experiment was only a partial success, that is to say, either station could read the other's signals (on Sir W. Thompson's Syphon Recorder) while holding down his key. The cause of failure to receive and send simultaneously was that the sending current at both stations affected the syphon so much as to obliterate the received signals, even when every other adjustment was at its best.

On the occasion of the second experiment, March 23rd, a modification of the above plan was adopted, viz: making the bridge unequal in the proportion of 1,000 to cable, 100 to artificial resistance, and condenser joined up as in Fig. 3. The condenser being 13 microfarads, the resistance 460 ohms, the battery 25 cells.

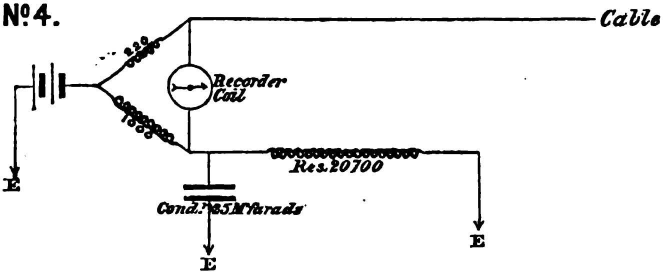
No. 3.



By these means we succeeded in reading a few words on the Syphon Recorder while sending to Lisbon, but Lisbon was unable to adjust his instruments, as, in order to lessen the injurious jerk on our own instrument, we adjusted, regardless of Lisbon's balance or adjustment.

Third experiment, March 30th: In this trial an unequal bridge was again used, in the proportions of 220 ohms to cable, and 1,000 G resistance and condenser, thus:—

No. 4.



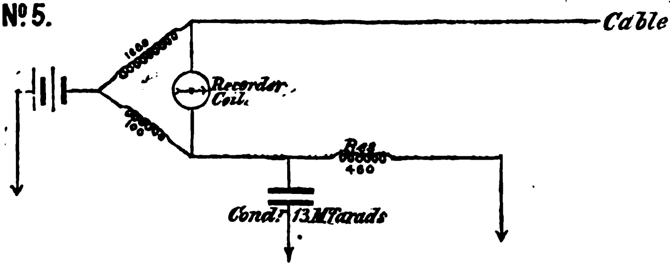
having resistance 20,700 ohms; condenser 25 microfarads; battery 5 cells—afterwards increased to 10 cells.

The reasons for using the above combination were, that in using the bridge for determining inductive capacity, the balance must be so arranged that to obtain no deflection on discharge, the greater resistance must be on the same side as the smaller capacity—the opposite arrangement being the case in resistance measurement.

This arrangement did not afford any improvement, the "kick" or jerk of the syphon, on sending, being as injurious as ever.

The fourth experiment on April 6th, was arranged as follows:—

No. 5.

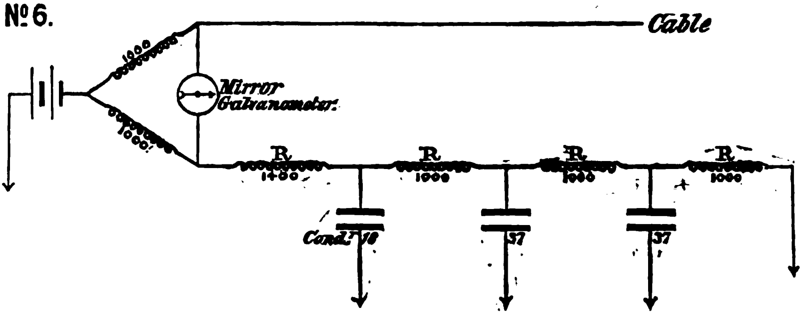


Bridge 1,000 to cable, 100 to resistance and condenser. Resistance 460 ohms, condenser 13 microfarads. Lisbon managed to read three messages whilst sending to Gibraltar, but at the latter place nothing readable was obtained; but still the experiment proved that by one station being able to work the system, it was probable that both could do so when the correct resistances and capacities were ascertained. Lisbon managed to get rid of the jerk of the needle (having at that end used a mirror galvanometer) almost entirely, only a slight tremble remaining.

The fifth experiment was made on Good Friday, April 11th, from the Cable House, at Camp Bay, so as to avoid any influence from the buried land lines.

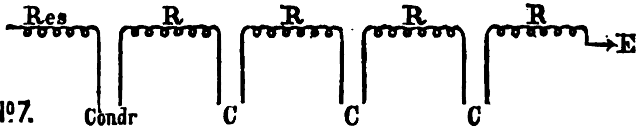
The instruments were joined up thus:—

No. 6.



Bridge 1,000 to cable and 1,000 to resistance and condenser Resistance, total 4,400 ohms. Condenser, total 92 microfarads.

Mr. Harwood (at Lisbon) had suggested arranging the condensers and resistances so as to represent an artificial cable as nearly as possible, thus:—



but though this special arrangement does *not* represent a cable—the suggestion to do so being good, we arranged the resistances and condensers as in Fig 6.

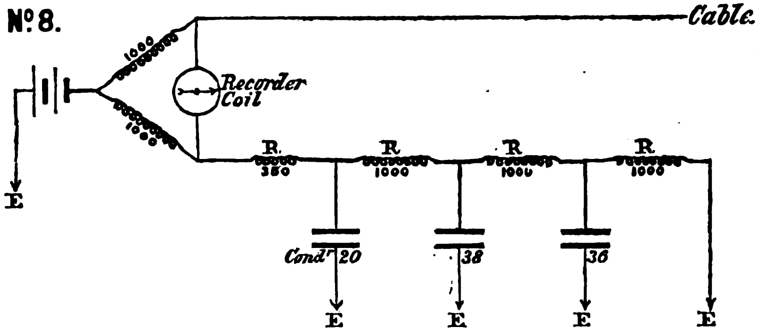
With this arrangement, Lisbon was able to read off (after making several modifications at his end) several messages whilst sending to Gibraltar, but at the latter place we were unable to read Lisbon's signals while sending to him, through the continued existence of the jerk on our instrument. By altering any of our capacities or resistances we succeeded in doing the same—*i.e.*, read while sending; but this threw out of adjustment the instruments at the other end.

Sixth experiment, April 13th—On commencing the experiments this day, as the copper resistance of the conductor varied with opposite poles, it being with zinc to line 3,014 ohms, with copper to line 5,608 ohms, two series of cells, bearing the same relative proportions to each other as the above resistances were tried; but instead of improving the signals, the opposite effect was obtained—

so that we returned to the plan, *Fig.* 2 at both ends of the cable. Lisbon was again able to read our signals when sending to us, but we were unable to read his without disregarding Lisbon's adjustment, and thus preventing him from reading while sending.

Seventh experiment, April 27th—This day's experiments resulted in perfect success. The arrangement was similar to that used on the fifth experiment, April 11th:—

No. 8.



Equal bridge of 1,000 ohms. Resistance total 3,540 ohms. Condenser total 94 microfarads. Battery 10 cells.

Almost immediately after joining up at Gibraltar, we obtained a perfect balance as regards resistance and inductive capacity. Our own signals were reduced to a faint irregular wave, as to be almost invisible; while Lisbon's signals were as clear and distinct as when we were not sending—the results as to marks being a complete success.

Lisbon succeeded in adjusting after a few trials, first on the Mirror Galvanometer, and afterwards on the Syphon Recorder, and we continued exchanging messages, working at both ends simultaneously at any speed, without the slightest hitch or hesitation—proving the possibility of Duplex Telegraphy on Submarine Cables up to a length of 360 knots.

The regular work of both stations was then conducted by its means for several hours, and would have been continued had our supply of instruments allowed us—as for these experiments we had to make use of condensers, etc., actually required on other circuits.

Although, I have only sketched out the guiding plan on each separate experiment, every possible modification was tried in batteries, condensers and resistances. I can only account for our failure on the

fifth experiment, by supposing that a faulty connection existed either at Gibraltar or at Lisbon, as that plan ultimately succeeded.

I think it probable that, when we have gained a little more experience in working the Duplex system, it will be possible to arrange the bridge unequally, so that smaller capacities than those quoted will give good results.

An evident, and as I consider, a great improvment in joining up Reversing Morse, Mirror Galvanometer, or Syphon Recorder Instruments, suggested itself in the course of our experiments, and that is the doing away with the switch, used to enable a station to send or receive. The neglect of turning the switch after sending, repeatedly causes great loss of time, so much so, that I was induced some years ago to arrange what I termed a Moral Morse Key, in which the switch to change from "send" to "receive," was compulsorily used by the clerk on taking hold of the knob of the Morse key—see Sabine's "Electric Telegraph, 1867."

Another evident improvement presented itself, that is:—by arranging the instruments at each end of a line according to any of the preceding diagrams, as if intending to work Duplex, the receiving station can immediately stop or interrupt the sending station whenever he cannot read through ink or signals failing, or paper running out—thus saving time. The expense of doing this need be very small.

During my earliest experiments on the Duplex system, in 1855, I experienced great difficulty in keeping the instruments in adjustment, owing to the inability of securing the constancy of the batteries employed, and to the loss of insulation on the line in wet weather. I believe that this latter trouble yet affects the perfect working of the Duplex system in England, especially when bad weather exists on one part of the line, the rest of the line passing through country where fine weather prevails.

Fortunately, in the Minotto Cells, we have a battery that is capable of being kept in such a state of permanency and constancy as to cause us no trouble; and equally fortunately, a varying state of insulation of the cable, to an extent sufficient to create any difficulty, is most likely to occur; but there is one difficulty that it is impossible to avoid seeing, and that is, the ever-varying line or earth currents.

As, however, no system of telegraphy can escape these effects, and we are able to nullify them to a great extent, it is not improbable that the Duplex system may be protected similarly from this source of trouble.

The successful issue of these experiments is mainly due to Mr. W. G. Taylor, M.S.T.E., and to Mr. Harwood, Superintendent at Lisbon, who were both more sanguine of success than I was, and who showed the greatest perseverance and courage in attacking every difficulty; to Mr. W. H. Preece, not only for his first suggestion, but for his continued advice, and to Mr. Ansell, M.S.T.E., Traffic Superintendent, Eastern Telegraph Company, who, on receiving the discouraging reports of our first attempts, by his stimulating advice to keep pegging away at the difficulties, as he was sure of ultimate success, my thanks are greatly due, and are hereby tendered.

NOTE.—Since the above was written, subsequent experiments by M. de Sauty, have proved highly satisfactory with condensers and resistances more sub-divided—W. T. ANSELL.

ABSTRACTS AND EXTRACTS.

ON THE ACTION OF A CONDUCTOR ARRANGED SYMMETRICALLY ROUND AN ELECTROMETER.

By Ch. V. Zenger.

I have the honour to address to the Academy the result of some fresh experiments on the electric inertia of a conductor, arranged symmetrically round an electroscope.

Ruhmkorff found that if static electricity exercises no action on the electroscope, disposed as I have indicated, it is not so with dynamic electricity, or the electricity of induction.

This result is only a confirmation of my theory of electric inertia, since the condition of equal distribution (equal superficial tension), and symmetrical, is not fulfilled when induction-apparatus is used. In fact the tension of the current after the opening and after the closing of the inducing current is not the same, and the charge of the symmetrical conductor is successively positive and negative; the superficial tension cannot be none, nor even equal, since a certain time is required for the two electricities to combine after two alternate unequal discharges, considering the tension and the nature of the electricity. The tension at the part of the symmetrical conductor furthest from the point of discharge will be quite different from that at the part nearest to the conductor of the Ruhmkorff; and the condition of equal superficial tension at every point of the symmetrical conductor is not fulfilled. Failing this essential condition, there will be an action nearly equal to the difference of tension of the sparks of opening and of closing.

To show the influence of the symmetrical distribution, I put symmetrically round a gold-leaf electroscope a rectangular copper wire; the electroscope and the wire are placed upon the brass plate of another electroscope (with straws instead of gold leaves), larger and less sensitive. From the conductor of an electrical machine strong sparks go to one of the angles of the wire; the upper electroscope shows not a trace of tension, while the straws of the large electroscope below are strongly affected.

If the experiment be modified by placing the knob of the upper electroscope not symmetrically in relation to the middle points of the sides of the conductor, there will be seen a movement of the gold leaves at every discharge from the conductor of the machine. The greater this defect of symmetry, the more sensible will be the action.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxv., p. 1765.

ON THE ELECTRICAL RESISTANCE OF METALS.

By M. Benoist.

It has long been known that the electrical resistance of metals increases as their temperature rises. This increase has been measured up to 100° by M. Becquerel and by Matthiessen, and to 200° in some metals by M. Lenz and, more recently, by M. Arndtsen. I proposed to myself to trace the variation beyond these limits, and to determine the increment of specific resistance at very high temperatures.

Calling x the specific resistance of a metal (that is to say, its resistance in unit length and unit section), the resistance of a wire of the same metal of length l and section s is, according to Davy's laws,

$$R=x\frac{l}{s},$$

or, substituting for s its value as a function of the volume V, weight P, and density D, of the wire,

$$R=\frac{xl^2}{V}=\frac{xDl^2}{P}.$$

If D, P, and l are known, and if R at $t°$ be determined, the value of the specific resistance at that temperature can be deduced from the last relation.

To measure R, I have chiefly employed the differential-galvanometer method of M. Becquerel. The current from two Daniell's elements was divided into two equal parts, which passed, in opposite directions, through the two wires of a very delicate differential-galvanometer. The wire to be studied was intercalated in one of the circuits, in the other a length of the wire of a rheostat, of which the resistance was equivalent when the needle was at zero. The resistances R, R′, ... of different wires submitted to experiment were proportional to the lengths l, l', ... of rheostat-wire which had served to measure them; and in order to express them as functions of a given unit, it was sufficient if the ratio of the rheostat-wire itself to this unit had been determined once for all. The rheostat consisted simply of two identical, very regular platinum wires, stretched parallel on a horizontal rule of two metres length. These wires traversed a cork cup containing mercury, carried by a cursor movable along the rule. The current, arriving through the first wire, traversed the mercury and issued through the second wire. On the rule was a scale of millimetres; and shifting the cursor n divisions increased or diminished the length of the circuit the value of $2n$. I shall not dwell upon the details, which permitted great precision to be attained, nor on the verifications which I made of the method and of the apparatus.

The wire under examination was soldered at each end to a copper rod, then wrapped round a cylinder of pipe-clay, and, finally, heated in a narrow and deep muffle which occupied the axis of a large wrought-iron jar. This was placed in a gas-stove with two concentric envelopes; by introducing a

suitable volatilizable substance, and heating to ebullition, the whole apparatus, and consequently the wire, was brought to a fixed and known temperature.

By determining thus the resistances of one and the same metal at various known temperatures, a number of points are obtained, from which the *curve of the resistances* can be constructed and its elements calculated.

The following are the temperatures which served for my determinations:—

Ebullition of water .	100°	Ebullition of sulphur .	440°
„ mercury	360°	„ cadmium .	860°

I made, besides, a great number of measurements below 360°, the apparatus being filled with mercury and heated by a regular current of gas; the temperature was indicated by thermometers placed in the muffle at different depths.

The results obtained by the preceding method were controlled and confirmed by determinations with Wheatstone's bridge, with the aid of a set of resistances similar to those employed in telegraphy.

The results are summed up in the Tables which follow.

In the first the conductivities at zero are expressed as functions of the two units which are now-a-days usually employed:—the theoretic absolute unit, or *ohm*, proposed by the British Association; and the mercury unit, adopted by M. Werner Siemens. The third column gives the ratios of the conductivities to that of silver, in order that the results may be compared with the well-known co-efficients of MM. Becquerel, Lenz, Matthiessen, &c.

The second Table gives the formulæ of the increment of the resistance with the temperature. This takes place regularly up to the melting-point, following the ordinates of a curve the abscissæ of which represent the corresponding temperatures, and which generally differs very little from a right line. Comparing the resistances to the resistance at zero, they can be expressed by a formula of the form

$$R^t=R_0(1+at+bt^2).$$

The constants a and b were calculated by the method of least squares, which makes all the observations co-operate for the determination of the most probable values of the unknown quantities.

The increment varies from one metal to another. In steel and in iron the initial resistance is doubled at about 170°; in silver, copper and gold, at about 255°; in platinum, at about 455°. In alloys the increment is in general less: in German silver, for example, at 860° the resistance has increased by only 0·3 of its value at zero. The numbers in this Table express the variation of the *specific resistance—i.e.* of the resistance reduced in each case to the unit of length and section. If we wish to use them to calculate the resistance at $t°$ of *a given wire* the resistance of which at zero is known, account must be taken of the changes of dimension of the wire; in other words, the resistance obtained must be multiplied by $\frac{1}{1+\delta t}$, δ being the co-efficient of dilatation. This correction cannot be neglected when the temperature exceeds certain limits.

Specific Resistances of Metals at Zero.

	Resistance of 1 metre length and 1 square millimetre section.		Conductivities compared with silver.
	ohm.	Siemens unit.	
Pure silver, annealed	0·0154	0·0161	100
Copper, annealed	0·0171	0·0179	90
Silver ($\frac{750}{1000}$), annealed	0·0193	0·0201	80
Pure gold, annealed	0·0217	0·0227	71
Aluminium, annealed	0·0309	0·0324	49·7
Magnesium, cold-beaten	0·0423	0·0443	36·4
Pure zinc, annealed at 350°	0.0565	0·0591	27·5
Pure zinc, cold-beaten	0·0594	0·0621	25·9
Pure cadmium, cold-beaten	0·0685	0·0716	22·5
Brass, annealed	0·0691	0·0723	22·3
Steel, tempered	0·1099	0·1149	14·0
Pure tin	0·1161	0.1214	13·3
Aluminium bronze	0·1189	0·1243	13·0
Iron, tempered	0·1216	0·1272	12·7
Palladium, tempered	0·1384	0·1447	11·1
Platinum, tempered	0·1575	0·1647	9·77
Thallium	0·1831	0·1914	8·41
Pure lead	0·1985	0·2075	7·76
German silver	0·2654	0·2755	5·80
Pure mercury	0·9564	1·0000	16·1

Variation of Resistance with Temperature.

Steel	$R^t = R_0$	$(1+0{\cdot}004978t+0{\cdot}000007351t^2)$
Iron	„	$(1+0{\cdot}004516t+0{\cdot}000005828t^2)$
Tin	„	$(1+0{\cdot}004028t+0{\cdot}000005826t^2)$
Thallium	„	$(1+0{\cdot}004125t+0{\cdot}000003488t^2)$
Cadmium	„	$(1+0{\cdot}004264t+0{\cdot}000001765t^2)$
Zinc	„	$(1+0{\cdot}004192t+0{\cdot}000001481t^2)$
Lead	„	$(1+0{\cdot}003954t+0{\cdot}000001430t^2)$
Aluminium	„	$(1+0{\cdot}003876t+0{\cdot}000001320t^2)$
Silver	„	$(1+0{\cdot}003972t+0{\cdot}000000687t^2)$
Magnesium	„	$(1+0{\cdot}003870t+0{\cdot}000000863t^2)$
Copper	„	$(1+0{\cdot}003637t+0{\cdot}000000587t^2)$
Gold	„	$(1+0{\cdot}003678t+0{\cdot}000000426t^2)$
Silver ($\frac{750}{1000}$)	„	$(1+0{\cdot}003522t+0{\cdot}000000667t^2)$
Palladium	„	$(1+0{\cdot}002787t+0{\cdot}000000611t^2)$
Platinum	„	$(1+0{\cdot}002454t+0{\cdot}000000594t^2)$
Brass	„	$(1+0{\cdot}001599t)$
Aluminium bronze	„	$(1+0{\cdot}001020t)$
German silver	„	$(1+0{\cdot}000356t)$
Mercury	„	$(1+0{\cdot}000882t+0{\cdot}000001140t^2)$

—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 342—346.

ON THE CONDITIONS REQUISITE FOR THE MAXIMUM OF RESISTANCE OF GALVANOMETERS.

By M. Th. Du Moncel.

Mr. Schwendler, and several other physicists previously, have found that, for a galvanometer to be in the best possible conditions of sensitiveness in relation to a circuit of given resistance, the resistance of its magnetizing helix must be equal to that of the exterior circuit in communication with it. Several experiments having demonstrated to me that the sensibility increases with the length of the helix-wire, under other conditions than those thus indicated, I submitted to calculation the galvanometric effects with regard to a circuit of given resistance; and I have ascertained that those conditions of sensitiveness demand a considerably greater length of wire in the multiplier than that which corresponds to the resistance of the exterior circuit.

To demonstrate the law which he had laid down, Schwendler endeavours to calculate the number t of the turns of the helix of the multiplier as a function of the space C occupied by the helix-wire, and also as a function of the resistance H of the latter. Designating by s the section of this wire, t becomes equal to $\frac{C}{s}$, and the length H of the helix equal to $\frac{Ct}{s}$; which supposes wrongly that the resistance is proportional to the number of the spiral turns, and inversely as the section of the wire.

According to these data, it would result from the combination of the values of t and H that t would be expressed by $\sqrt{H}$; and as, besides the value of the intensity of the current is $\frac{E}{R+H}$ (R being the resistance of the exterior circuit, E the electromotive force of the source of the electricity), the magnetic moment F of the needle would be

$$F = \frac{E\sqrt{H}}{R+H},$$

an expression susceptible of a maximum for $R = H$.

But in reality the value of t is far from being expressed by $\sqrt{H}$; and if we amend the preceding formula by inserting the true quantities, we arrive at altogether different conditions.

In fact, let a be the thickness of the layers of spires, b the width of the galvanometric framework, c the diameter of the circular part, which terminates it on each side, d the length of the framework between these two circular portions, and g the diameter of the wire (including its insulating covering); the number of the turns t will be expressed by $\frac{ab}{g^2}$, and the length H of the helix by

$$H = \frac{ab}{g^2}\left[(a+c)\pi + 2d\right];$$

consequently the magnetic moment F of the needle will be

$$F=\frac{\alpha E a b}{g^2R+ab[(a+c)\pi+2d]},$$

α denoting the constant of the instrument. Now the conditions of a maximum for this formula, taking a for variable, which is the sole quantity proportional to t, lead to the relation

$$R=\frac{\pi b a^2}{g^2},$$

which shows that the resistance of the galvanometer-wire must be greater than that of the exterior circuit by a quantity represented by

$$\frac{ab}{g^2}(\pi c+2d).$$

We can, moreover, easily assure ourselves of the truth of this deduction by supposing the framework of the galvanometer represented by a simple coil, as in the Thomson galvanometer, and taking the magnetic moments of the needle with the two lengths of the wire of the helix corresponding to the two conditions for a maximum given by Mr. Schwendler and me. For greater simplicity, we will represent the diameter c by $2r$. In these conditions the length H of the helix, instead of being greater than the resistance R by a quantity represented by $\frac{ab}{g^2}(\pi c+2d)$, will be greater only by a quantity $\frac{ab}{g^2}\pi c$, or will be in proportion to R, as $a+2r$ is to a. We shall therefore have:—

1. With $H=R$,

$$F=\frac{Eab}{3\pi ba(a+2r)};$$

2. With $H=R\frac{a+2r}{a}$,

$$F'=\frac{Eab}{2\pi ba'(a'+r)},$$

which leads to

$$\frac{F'}{F}=\frac{a+2r}{a'+r}.$$

And as then we have $\frac{\pi ba(a+2r)}{g^2}=\frac{\pi ba'}{g^1}$, we deduce $a'=\sqrt{a(a+2r)}$, and consequently

$$\frac{F}{F}=\frac{a+2r}{\sqrt{a^2+2ar}+r}.$$

As $a+2r$ can be put in the form $\sqrt{a^2+2ar+r^2}+r$, it is at once seen that F′ is greater than F.

The experimental verification of the above-exhibited deduction not being easy to realize, on account of the too great sensitiveness of galvanometers with resistant helices and with continual variations of the resistance of the exterior

circuit with galvanometers of short helix, I made the experiment with electromagnets, the attractive force of which, reckoned according to the laws of MM. Dub and Jacobi, admits of the same conditions of a maximum relative to the resistance of the magnetizing helices, as I have shown in my researches on the best construction of electromagnets. Now the following are the results I obtained with one and the same electromagnet excited by a Daniell pile of 20 elements, to which I applied successively magnetizing coils of two different resistances, viz., a resistance of 75 kilometres of telegraphic wire of 4 millims. diameter, and a resistance of 200 kilometres. In order to avoid static reactions, the attractive forces were measured with 1 millimetre distance of separation from the armature.

(1) Force of the electromagnet with coils of 75 kilometres resistance:—

	metres.	metres.	Attractive force grammes
The exterior circuit having	18620+	0	80
" " "	18620+	100000	15
" " "	18620+	200000	5.5
" " "	18620+	370000	0

(2) Forces of the electromagnet with coils of 200 kilometres resistance:—

The exterior circuit having	18620+	0	58
" " "	18620+	100000	25
" " "	18640+	200000	14
" " "	18620+	370000	0

The wire of these circuits was perfectly insulated; and the time during which the current was closed was long enough to develope the maximum of magnetization.

Now we see by these numbers that it is when the resistance of the coils is much superior to the exterior resistance that the maximum is attained, and that, *for the exterior circuit of* 100 *kilometres, the maximum is obtained with the helices of* 200 *kilometres*—that is to say, helices presenting double the resistance. Calculation leads to the same deduction; for in the electromagnet the thickness a of the helix was equal to the diameter c of the electromagnet, and consequently the value of H, which must be equal to $R\frac{a+c}{a}$ to satisfy the conditions of a maximum, was found, under the circumstances of my experiments, to be equal to 2R.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxvi. pp. 368-371.

THE ACTION OF LIGHT ON THE ELECTRICAL RESISTANCE OF SELENIUM.

By Lieut. Sale, R.E.

It having been recently brought to notice that selenium in the crystalline condition exhibits the remarkable property of having a conductivity varying with the degree of light to which it is exposed, the following experiments were undertaken with a view to the further elucidation of the matter:—

Experiment 1.—A bar of crystalline selenium measuring approximately 1·5″×·5″×·05″ was procured, and platinum wire terminals were fastened to the ends.

The bar itself was then enclosed in a box having a draw-lid, so as to admit or exclude the light at pleasure.

Then, the lid of the box being on, the resistance of the selenium was measured by means of a high-resistance galvanometer and a Wheatstone's bridge, with dial-coils capable of measuring up to 10,000,000 ohms. The battery-power was 2 cells Daniell.

The measurement was made on a dull cloudy day, and in a room of equable temperature.

The resistance having been carefully balanced, the lid of the box was withdrawn, when the resistance of the selenium fell instantaneously and considerably as indicated by the rapid movement of the spot of light on the galvanometer-scale.

Experiment 2.—The transition from darkness to the light given by an ordinary gas-burner (conditions as before), caused a slight and barely perceptible fall in the resistance.

Experiment 3.—The bar of selenium was next tried in the solar spectrum on a very bright cloudless day (conditions as before), except that more battery-power was used (10 cells Daniell).

The diffused daylight could not be cut off; so the trial was made in the most shaded part of an ordinary room, the spectrum being superimposed on the ordinary diffused daylight.

The resistances were very carefully balanced in each case, with the following results:—

Resistance in darkness	330,000
„ „ violet	279,000
„ „ red	255,700
„ „ orange	277,000
„ „ green	278,000
„ „ blue and indigo	279,000
„ „ centre of red	255,000
Resistance just on the outside edge of red, red side	220,000
Resistance in dark rays clear of red	228,000
Resistance in diffused daylight only	270,000
Resistance taken in the dark immediately after exposure (resistance rising)	310,000

The indications were very clear; and the bar of selenium was so sensitive to the action of the spectrum, that a slight movement of the prism produced a corresponding movement in the spot of light on the galvanometer-scale.

It is to be noted that in this experiment the reflecting galvanometer was

placed on a heavy masonry pillar insulated from the floor for observatory purposes, and that the battery, bar of selenium, and resistance-coils were in another room, being connected by long and carefully insulated leads with the galvanometer.

Experiment 4.—The diffused light was cut off as much as possible by screens; and the resistances were again balanced in the solar spectrum. Conditions as in the last case.

Resistance of selenium in red	240,000
,, ,, just outside red	240,700
,, ,, in blue....................................	270,000
,, ,, in such diffused light as came through screens	290,000
Light cut off by lid of box (resistance rising)	310,000

Experiment 5.—The selenium was also exposed to the spectrum of the electric light in a darkened room.

The effect was feeble; but by using more battery-power in balancing, it was possible to measure the swing of the spot of light when the selenium was suddenly exposed to the action of the light of the spectrum.

The maximum effect was obtained in, or just at the edge of the red, the violet and blue rays producing scarcely any effect.

Epxeriment 6.—The selenium was exposed to the full sunlight; the resistance fell enormously and instantaneously, and on balancing it was found to be little more than half what it was in the darkness.

The following were the general results of the experiments :—

Results.—(1) That the resistance of selenium is largely affected by exposure to light.

(2) That this effect is not produced by the actinic rays, but is at a maximum at, or just outside the red rays, at a place nearly coincident with the locus of the maximum of the heat-rays.

(3) That the effect of varying resistances is certainly not due to any change of temperature in the bar of selenium.

(4) That the effect produced on exposure to light is sensibly instantaneous, but that, on cutting off the light, the return to the normal resistance is not so rapid.

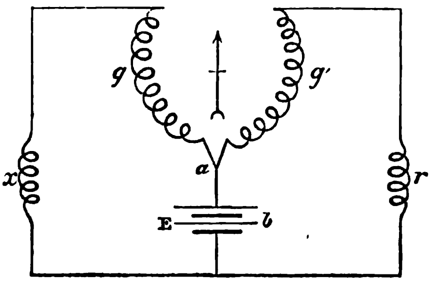
It would seem that there exists a power in rays nearly coincident with the heat-rays of high intensity, of altering instantaneously and without change of temperature the molecular condition of this particular element.

ON AN ADVANTAGEOUS METHOD OF USING THE DIFFERENTIAL GALVANOMETER FOR MEASURING SMALL RESISTANCES.

By Oliver Heaviside, Great Northern Telegraph Company, Newcastle-on-Tyne.

In the usual method of measuring resistances with the differential galvanometer, the current from the battery is divided between the two coils, having opposite effects on the needle within them, so that, if the currents in both the coils are equal, the needle is unaffected. The introduction of resistance in the circuit of one coil will not affect the balance, provided an equal resistance is introduced in the circuit of the other coil. Hence, if on one side we place a rheostat, and on the other an unknown resistance, the latter may be determined by varying the resistance of the rheostat until a balance is obtained. Fig. 1 is a representation of this arrangement. The current from the battery, having a resistance b and electro-motive force E, divides at the point a between the coil g and resistance x, and the coil g' and resistance r. When $r=x$, the needle is unaffected.

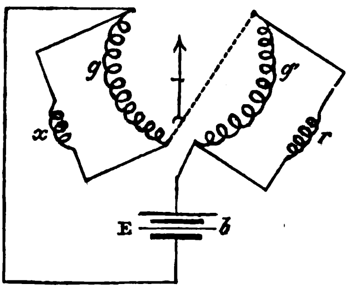
Fig. 1.



By the following method of using the differential galvanometer, a much greater accuracy is obtained when the unknown resistance whose value has to be determined is small.

Instead of dividing the current from the battery E between the two coils, I join up the coils, so that the same current passes through both of them, and by reversing one of the coils, g', prevent the current from influencing the needle (see fig. 2). The rheostat r is connected to the two ends of one coil, and the resistance to be measured, x, to the two ends of the other. It will easily be seen, without further explanation, that when $r=x$, the currents in g and g' are equal; but should r not equal x, there will be a greater current in one coil than in the other, and the needle will move in obedience to the difference of these currents. It then only remains for me to show what, and under what circumstances, advantages are obtained by this method. To do so, we have only to compare the expressions for the difference-currents in the two methods.

Fig. 2.



By the first method the resistance external to the battery is

$$\frac{(x+g)(r+g)}{x+r+2g}$$

therefore the current from the battery is

$$B=\frac{E}{b+\frac{(x+g)\,(r+g)}{x+r+2g}}=\frac{E\,(x+r+2g)}{b\,(x+r+2g)+(x+g)\,(r+g)}.$$

This current divides between the two paths $x+g$ and $r+g$ in inverse proportion to their resistances; therefore the current in g is

$$G=B\times\frac{r+g}{x+r+2g}=\frac{E(r+g)}{b(x+r+2g)+(x+g)(\,r+g)},$$

and the current in g' is

$$G'=B\times\frac{x+g}{x+r+2g}=\frac{E(x+g)}{b(x+r+2g)+(x+g)\,(r+g)}.$$

The effective current (that influencing the needle) will be the difference of G and G′, say

$$D_1=\frac{E(r-x)}{b(x+r+g2)+(x+g)\,(r+g)}. \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

By the second method, the resistance external to the battery is

$$\frac{xg}{x+g}+\frac{r+g}{r+g};$$

therefore the current from the battery is

$$B=\frac{E}{b+\frac{xg}{x+g}+\frac{rg}{r+g}}=\frac{E(x+g)\,(r+g)}{b(x+g)\,(r+g)+gx(g+r)+gr\,(g+x)}.$$

The current in g is

$$G=B\times\frac{x}{x+g}=\frac{Ex\,(g+r)}{b\,(x+g)\,(r+g)+gx(r+g)+gr(x+g)},$$

and the current in g'

$$G'=B\times\frac{r}{r+g}=\frac{Er\,(g+x)}{b\,(x+g)\,\,(r+g)+gx(r+g)+gr(x+g)}.$$

The effective current will therefore be

$$D_2=G-G'=\frac{Eg(x-r)}{b\,(x+g)\,(r+g)+gx(r+g)+gr(x+g)}. \quad . \quad . \quad . \qquad (2)$$

Equations (1) and (2) give the effective current in each case; and we may ascertain the relative sensitiveness of the two methods by comparing D_1 and D_2.

$$\frac{D_2}{D_1}=\frac{b\,(x+r+2g)+(x+g)\,(r+g)}{b\,(x+g)\,\,(r+g)+gx(r+g)+gr(x+g)}\times g;$$

and in the limit, when $x=r$,

$$\frac{D_2}{D_1}=g\times\frac{2b+r+g}{b\,(r+g)+2gr}.$$

When $r=g$, $\frac{D_2}{D_1}=1$, showing that the two methods are equally sensitive for that value of r or x which equals the resistance of one coil of the galvanometer. When r is greater than g, the ordinary method is to be preferred, for $\frac{D_2}{D_1}$ is then less than unity. It can, however, never be less than $\frac{g}{b+2g}$, which happens when r is infinite.

But for values of r less than g, $\frac{D_2}{D_1}$ is greater than unity, and increases rapidly as r is reduced, until in the limit, when $r=O$, $\frac{D_2}{D_1}=2+\frac{g}{b}$.

This proves that when the resistance to be measured is smaller than that of the galvanometer-coil, the second method is much to be preferred. For instance, let the battery have a resistance of 10 ohms, the galvanometer (each coil) 500 ohms, and $r=10$ ohms, then the second method is 17 times as delicate as the first; and if r were 1 ohm, the second method would be 416 times as delicate.

In fact, if, after getting as true a zero as possible by the ordinary method, the connections be altered to the second arrangement, the slight inequality between r and x, which was inappreciable by the ordinary method, will be at once rendered evident by a large deflection of the needle.

SOUTH AMERICAN TELEGRAPHY.

A FEW days ago Mr. Burton congratulated President Sarmiento on having 6,000 miles of telegraph wire open to public service throughout the Republic. No other country in South America comes near us in this particular, and we may venture to say that only three or four Kingdoms in Europe can boast so many miles of wire, while none can show so rapid a development, or so cheap a system of telegraphs. You can send a message from Buenos Ayres a thousand miles, to the frontiers of Bolivia or Paraguay, for one shilling! The immense utility of this service is shewn by the enormous increase of business, obliging Mr. Burton to solicit permission to put on extra wires, and it would seem as if the telegraph is going to supplant the post office system throughout the Interior. A merchant of Tucuman or San Juan can now telegraph to Buenos Ayres and obtain an answer in a couple of hours, for half-a-dollar, instead of writing by letter and waiting ten or twenty days for a reply.

Thanks to telegraph wires, also the National Government is able to prevent revolutions in the remote provinces, and if we had a complete network of telegraphs on all parts of our Indian frontier it would be comparatively easy to check the savages at any point where they should venture to make an inroad

Doubtless it would keep the Minister of War very busy, and his Excellency would be forced to have his official Mayor sitting in the office all night to answer sudden applications. Indeed, looking at the incalculable benefits from telegraph wires it is not so much to be wondered at that we have now 6,000 miles of service, but rather that we had not as many ten years ago.

The Brazilians have not been so fortunate with their telegraph lines, although it would seem of primary necessity to have communication between the metropolis and the various provinces of so vast an empire: the line from Port Alegre northwards is interrupted two hundred days every year by trees falling on the wires, or similar mishaps. In the Argentine Provinces interruptions are rare, and the lines are so easy of construction that the outlay never exceeds 500 dols. fts. per mile, say three million dollars for the total length now in working order.

But we are indebted to Brazil for the great enterprise of an ocean cable to Europe, which will be twice as long as the cables from England or France to North America. The "Great Eastern" is now getting under weigh to proceed with the first section to Madeira, and the whole line to Pernambuco is to be ready for service by May, 1874. Baron Maua and his friends met with such ready assistance in London that more than the necessary capital was subscribed the moment the lists were opened, and the West Indian Telegraph Company transferred its intended scene of operations to the coast of Brazil, the cable being already coiled for shipment which is to unite Pernambuco to Maldonado.

Thus, within twelve months the River Plate will be in immediate communication with Europe; the price of messages will be very high, say £10 for ten words, the Brazilian concession allowing the company to charge 4 dols. per word from Rio to Lisbon. When the Atlantic cable began working the charge was £10 a message, but it soon fell to £5, and gave larger profits: at present, we believe, it is only £2 for ten words. Although Baron Maua and his friends have a monopoly from Brazil it is possible the success of his cable will lead to another being laid down from the River Plate to Europe, especially as the Chilian traffic must pass over our wires.

Compare the rates of various countries more remote than ours from England. The tariff for 20 words is, to—

East Indies	£4	5	0
Japan	7	0	0
Australia	9	17	6

If we go by distance the charge for 20 words from Buenos Ayres to London should not be over £5, and we may hope that the Company will see the expediency of putting the rate rather under than over that figure, it must be borne in mind that as soon as the land wires are completed along the West Coast, from Panama to Valparaiso, we shall be able to send messages *via* Panama, Jamaica and the United States, to Europe. If the Maua cable establish anything like a moderate tariff even the Chilians and Peruvians will

prefer sending their messages *via* Buenos Ayres and Brazil, as the speedier and more secure route.

A considerable reduction will, no doubt, be allowed for press telegrams, as it will always be somewhat onerous for the new spirited newspaper proprietors that will have to bear all the expense of procuring a daily telegram from London. The difference of four hours in time will enable us to receive London advices up to 9 p.m., which will arrive here by 6 or 7 p.m., before going to press.

In a word the Maua cable will work quite a revolution among the mercantile and editorial world of South America, but however individuals may be incommoded the public will be better served.—"*The Standard*," *Buenos Ayres, April 15*, 1873.

ON THE ELECTRICAL STORM OF JANUARY 7TH AND 8TH.

By C. H. Haskins.

The *Telegrapher* published, some time ago, a paper by Mr. C. H. Summers, electrician, on the electrical storm of January 7 and 8, reaching from Central Iowa, in a north-easterly direction to Detroit, Mich. I have gathered a few facts relative to a storm of the same date that will interest your readers.

In northern Minnesota and northern Wisconsin, on the 7th of January, electrical disturbances were experienced on the lines of the Northern Pacific and the West Wisconsin Railroads.

The wind blew from the north-west, with the thermometer about zero and a driving snow storm. The storm continued for two days.

On the 8th the disturbance on the railroad wire of the Northern Pacific, between the Mississippi and Red Rivers, was so great that the train despatcher could not work his line. The battery current seemed entirely neutralized by the atmospheric current.

Finally, the operator at a station midway on the circuit put on his ground wire, and worked with the train despatcher's office at Brainerd, at Mississippi river crossing, without difficulty. The train despatcher, thinking he had taken his ground off again, called offices *west* of the ground wire, and worked well for a long time. He was astonished to learn that the ground at the intermediate office was *on*. As long as it was kept on he worked well, *right over it;* when it was taken off he could not work at all. This condition of things continued until the atmospheric current ceased. Then the ground wire was removed, and the line worked as usual.

Precisely the same thing was done, in the same way, on the West Wisconsin Railroad line, with the same results.

On the Green Bay and Lake Pepin Line, which extends from a station on

the West Wisconsin road, eastward, to Green Bay, Wis., the atmospheric current, flowing eastward, neutralized the battery current in the same manner. The telegraph superintendent, thinking the battery at the other end of the line had been reversed, as he could get no current except with a ground wire at some way station, reversed one of the terminal batteries. This, acting *with* the atmospheric current, made the line work well for a few hours. Then, when the atmospheric current became weak, gradually dying out, the two batteries, neutralizing each other, deprived the line of current again, and the reversal of the battery to its former direction restored things to their normal condition.

The current Mr. Summers noted flowed from the south-west to the *north-east*—the one I speak of from the north-west to the *south-east.* They would, therefore, meet and mingle at or near Detroit. On consultation with Mr. Summers we have both concluded that this is the fact, and that the currents were of opposite polarity, thus neutralizing each other when they met and mingled. Thus, beyond the north line of the Iowa storm, the influence was not perceptible, nor was it south of the southern border of the Minnesota storm. As they travelled eastward the breadth of country affected grew more narrow, until, where they converged at Detroit, the effect of the storm disappeared; and between the two storms the country unaffected narrowed also. Thus, at Milwaukee, no effect was perceived, while at Kenosha, Wis.—thirty-four miles south of Milwaukee—the wires were rendered useless for a short time by discharges of not only considerable tension but of apparently unusual quantity. Relay armatures were closed with a vigour only equalled by short circuiting a heavy main battery through them, yet not an instrument was injured in the least. The discharges crossed the points of a key open the usual distance, but when that distance was doubled the current had not sufficient tension to pass the space.

So far as I have been able to learn, not a pole was shivered or an instrument scorched during the entire storm.

I trust that every one having any facts relative to this matter will communicate them to your paper, that we may learn more of its route and peculiarities. Should Professor Tyndall succeed in his endeavour to make a simple serviceable instrument for testing the electrical condition of the atmosphere at the signal service stations throughout this country, we shall be enabled, I trust, to arrive at some definite knowledge relative to the tension, polarity and direction of these mysterious currents. At present we have a few isolated facts— all the rest is mystery.—*Telegrapher.*

ON THE ELECTRICAL PROPERTIES OF CLOUDS AND THE PHENOMENA OF THUNDER STORMS.

By Professor OSBORNE REYNOLDS, M.A.

THE object of this paper is to point out the three following propositions respecting the behaviour of clouds under conditions of electrical induction, and to suggest an explanation of thunder storms based on these propositions, and on the assumption that the *sun is in the condition of a body charged with engative electricity.*

1. A cloud floating in *dry* air, forms an insulated electrical conductor.

2. When such a cloud is *first* formed, it will not be charged with electricity, but will be ready to receive a charge from any excited body to which it is near enough.

3. When a cloud charged with electricity is *diminished* by evaporation, the tension of its charge will increase until it finds relief.

I do not imagine that the truth of these propositions will be questioned, but rather that they will be self-evident. However, as a matter of interest, I have made some experiments to prove their truth, in which I have been more or less successful.

Experiment 1 was to show that a cloud in dry air acts the part of an insulated conductor. The steam from a vessel of hot water was allowed to rise past a conductor, the apparatus being in front of a large fire, so that the air was very dry. When the conductor was charged, the column of vapor was deflected from the vertical to the conductor, both for a positive and negative charge.

Experiment 2 was made with the same object as experiment 1. A gold leaf electrometer was charged so that the leaves stood open, and then a cloud made to pass by the insulated leaves. As the cloud passed they were both attracted. This experiment was attended with considerable difficulty, as the moisture from the steam seemed to get on the glass shade over the gold leaves, and so form a charged conductor between the leaves and cloud. The cloud was first formed by a jet of steam from a pipe, then by the vapor from a vessel of boiling water, and lastly, by a smoke ring, or rather a steam ring. By this latter method, an *insulated* cloud was formed, which, as it passed, was attracted by the charged leaf.

Of the two latter propositions, I have not been able to obtain any experimental proof. I made an attempt, but failed through the bursting of a vessel in which the cloud was to be formed. I hope, however, shortly to be able to renew the attempt, and in the meantime I will take it for granted that these propositions are true. Faraday maintained that evaporation was not attended by electrical separation, unless the vapor was driven against some

solid, when the friction of the particles of water give rise to electricity. So that unless there were some free electricity in the steam or vapor before it was condensed, none could be produced by the condensation, and hence the cloud when formed would be uncharged.

In the same way, with regard to evaporation, unless, as is very improbable, the steam into which the water is turned retains the electricity which was previously in the condensed vapor, the electricity from that part of the cloud which evaporates must be left to increase the tension of the remainder. So that, as a charged cloud is diminished by evaporation, the tension of the charge will increase, although the charge remains the same.

I will now point out what I think to be the bearing which these propositions have on the explanation of thunder storms. In doing this, I am met with a great difficulty, namely: ignorance of what actually goes on in a thunder storm. We seem to have no knowledge of any laws relating to these everyday phenomena; in fact we are where Franklin left us—we know that lightning is electricity, and that is all.

It is not, I think, decided whether the storm is incidental on the electrical disturbance or *vice versa*, *i. e.*, whether the electricity causes the clouds and storm, or is a mere attendant on them. Nor can I ascertain that there is any certain information as to whether, when the discharge is between the earth and the clouds, the clouds are positive and the earth negative, or *vice versa*. Such information as I can get, appears to point the following law: that in the case of a fresh-formed storm, the cloud is negative and the earth positive whereas, in other cases, the cloud is positive and the earth negative.

Again, thunder storms move without wind, or independently of wind; but I am not aware that any law connecting this motion with the time of day, etc, has ever been observed, though it seems natural, that however complicated by wind and other circumstance, some such law must exist. In this state of ignorance of what the phenomena of thunder really are, it is no good attempting to explain them. What I shall do, therefore, is to show how the inductive action of the *Sun* would necessarily cause certain clouds to be thunder clouds in a manner closely resembling, and for all we know, identical with, actual thunder storms.

In doing this, I assume that the thunder is only an attendant on the storm, and not the cause of it; and that many of the phenomena, such as forked and sheet lightning, are the results of different states of dampness of the air, and different densities in the clouds and really indicate nothing as to the cause of electricity. In the same way, the periodicity of the storms, is referred to the periodical recurrence of certain states of dryness of the atmosphere. Thus the fact that there is no thunder in winter, is assumed to be owing to the dampness of the air, which allows the electricity to pass from and to the clouds quietly. What I wish to do, is to explain the cause of a cloud being at certain times, in a different state of electric excitation to the earth and other clouds, and of this

difference being sometimes on the positive side and sometimeson the negative, that is to say, why a cloud should sometimes appear to us on the earth to be positively charged, sometimes negatively, and at others not to be charged at all.

The assumed condition of the sun and earth may be represented by two conductors S and E, acting on one another by induction, the sun being negative and the earth positive. The distance between these bodies is so great that the inductive action would not be confined to those parts which are opposed, but would in a greater or less degree extend all over their surfaces, though it would still be greater on that side of E, which is opposite to S, than on the other side.

The conductor E must be surrounded by an imperfectly insulating medium to represent damp air. The formation of a cloud may then be represented by the introduction of a conductor C near to the surface of E. Such a conductor at first having no charge would attract the positive electricity in E, and appear by reference to E to be negatively charged. If it was near enough to E, a spark would at once pass, which would represent a flash of forked lightning. If it were not near enough for this, it would obtain a charge through the imperfect insulation of the medium. Such a charge might pass quietly, or by the electric brush. When the cloud had obtained a charge, it would not exert any influence on the earth, unless it altered its position. But if the heat of the sun caused part of the cloud to evaporate, the remainder would be surcharged and appear positive. Or if C approached E then C would be overcharged, and a part of its electricity would return, and on its return it might cause positive lightning. Thus, suppose that after a cloud had obtained its charge, part of it came down suddenly in the form of rain. As the rain came lower its electric tension would increase, until it got near enough the ground to relieve itself with a flash of lightning, almost immediately after which the first rain would reach the ground. It has often been noticed that something like this often takes place; it often begins to pour immediately after a flash of lightning, so much so that it seems that the electricity had been holding the rain up, and it was only after the discharge that it could fall. This, however, cannot be the case, for the rain often follows so quickly after the flash, that there would not have been time for it to fall from the cloud unless it had started before the discharge took place. If, on the other hand, C receded from E, it would again be in a position to accept more electricity, or would again become negative. In this way, a cloud in forming, or when first formed, would appear negatively charged; soon after it would become neutral, and then if it moved to and from the earth, it would appear positively or negatively charged.

If the air was very dry, as it is in the summer, any exchange of electricity between the earth and the cloud would cause forked lightning, in the winter it would take place quietly, by the conduction of the moist atmosphere.

In this way, then, there would sometimes be positive, sometimes negative lightning; sometimes the discharge would be a forked flash or spark, sometimes a brush or sheet lightning. And if clouds are formed in several layers

as would be represented by another conductor, D, outside C, then, in addition to the phenomena mentioned, similar phenomena would take place between C and D; and if, in addition to this, we were to assume that there are other clouds in the neighbourhood, the phenomena might be complicated to any extent.

And if, further, the motion of the sun is taken into account; as the conductor S moves round E the charges in D and E would vary, accordingly as they were more or less between S and E and directly under the induction of S, *i.e.*, the charge in a cloud would appear to change owing to the motion of the sun; thus, a cloud would appear to change owing to the motion of the sun; thus, a cloud that appeared neutral at mid-day would, if it did not receive or give off any electricity, become changed positively in the evening.

With regard to the independent motion of the clouds, there are several causes which would effect it. For instance, a cloud, whether it appeared on the earth to be negatively or positively charged, would always tend to follow the sun, though it is possible this tendency might be very slight. Again, one cloud would attract or repel another, according as they were charged with the opposite or the same electricities; and in the same way a cloud would be attracted or repelled by a hill, according to the nature of their respective charges.

Such, then, would be some of the more apparent phenomena under the assumed conditions. So far as I can see, they agree well with the general appearance of what actually takes place, but, as I have previously said, the laws relating to thunder storms are not sufficiently known to warrant me in doing more than suggesting this as a probable explanation.

In these remarks I have said nothing whatever about what is called atmospheric electricity, or the apparent increase of positive tension as we proceed away from the surface of the earth. I do not think that this has much to do with thunder storms. If the law is established, it seems to me that it will require some explanation, besides merely that of the solar induction acting through the earth's atmosphere on to the surface of the earth. It would rather imply that the sun acts on some electricity in the higher regions of the earth's atmosphere, and that electricity in these regions acts again on the surface of the earth; but, however this may be, the effect of the assumptions described in this paper would be much the same.—*The Telegrapher.*

A STRANGE STORY.

TO THE EDITOR OF THE TIMES.

Sir,—As the following incident relating to submarine telegraphy may interest a large portion of your readers, I send you the account, in the hope that you may think it worthy of publication in *The Times.*

The cable between Kurrachee and Gwadur (a distance of about 300 miles) suddenly failed on the evening of the 4th instant. The telegraph steamer Amber Witch, under the command of Captain Bishop (late I.N.), with the

electrical and engineering staff under Mr. Henry Mance, proceeded on the following day to repair the damage, which by tests taken at either end appeared to be 118 miles from Kurrachee. The Amber Witch arrived on the ground at 2 p m. on the 6th, a heavy sea and thick fog prevailing at the time, but the cable was successfully grappled within a quarter of a mile of the fault.

The soundings at the fault were very irregular, with over-falls from 30 to 70 fathoms. On winding in the cable unusual resistance was experienced, as if it were foul of rocks, but after persevering for some time the body of an immense whale, entangled in the cable, was brought to the surface, when it was found to be firmly secured by two and a half turns of the cable immediately above the tail. Sharks and other fish had partially eaten the body, which was rapidly decomposing, the jaws falling away on reaching the surface. The tail which measured fully 12 feet across, was perfect, and covered with barnacles at the extremities. The sea being too rough at the time to make use of the boats, an attempt was made to haul the whale on board, but its own weight broke it away from the injured cable as soon as it was raised above the surface.

Apparently the whale was at the time of entanglement using the cable to free itself from parasites, such as barnacles, which annoy them very much, and the cable, hanging in a loop over a submarine precipice, he probably, with a fillip of his tail, twisted it round him, and thus came to an untimely end.

During the time the Amber Witch remainded on the ground a large school of whales continued to play in the close vicinity of the ship, frequently blowing within a dozen yards of the vessel, and even rubbing themselves against the hawser by which the ship was secured.

Before the outbreak of the Civil War several American whalers regularly frequented the coast of Beloochistan, and were generally very successful in filling up with oil in a single season, but of late years the whale fishery in these waters has been abandoned.

To show how closely these whales can be approached, I may mention that I was coming in a native boat from Ormara to Kurrachee, a distance of 140 miles, on the 12th of November, 1862. The inhabitants of the coast looked on me as a jadoo-wallah, or magician, when first introducing the telegraph into their inhospitable country, and remembering the annual display of meteors that would take place about the time of my leaving Ormara, I told my wild boatmen to expect a shower of stars during the night. The meteoric display duly commenced about 1 a.m., and shortly afterwards an enormous whale came alongside and kept close company with the boat, now and then passing under it and grazing it with its back, much to our alarm. Having a double-barrelled rifle and a revolver under the cork-bed on which I was lying, I wished to fire at the whale, with a view to frighten him away, but the natives begged me not to do so, as he would destroy the boat; and vivid recollections of the pictures illustrating whale fishing, with a boat 30 feet in the air, bottom upwards, and the crew with their ropes, harpoon, &c., coming down head foremost, satisfied

me of the prudence of their advice. We made all haste to get into shallow water, my crew praying furiously and shouting to Allah for protection, when another whale came alongside, and the two kept us close company for two or three hours, until we got into some four fathoms of water. Had I not been well armed I should probably have been treated like Jonah, or disposed of in some equally unceremonious manner, although, perhaps, with a less fortunate result.

As the officers engaged in repairing the late fault and the boatmen who accompanied me in 1862 are all accessible, I must refer any sceptical readers to them for corroboration of my statement.

Kurrachee, July 8. H. IZAAK WALTON.

TELEGRAPHY IN ITS RELATION TO RAILWAY WORK.

(*Times.*)

Mr. W. H. Preece delivered a lecture before the Royal Cornwall Polytechnic Society on the evening of August 27th, upon "Telegraphy in its relation to railway work." Mr. Preece is telegraphic engineer to the Post-office for the Southern Division of England, and his lecture was abundantly illustrated by apparatus and experiments. He commenced by distinguishing between science and its application—the distinction being the same as that which existed between a tree and its fruit. No science had borne better fruit than electricity. Mr. Preece then proceeded to lay down and enforce three propositions—First, that railway travelling was actually safe; second, that it was passively dangerous; third, that its actual safety was due to the fact that science had stepped in and caused the dangers to lie dormant; the term science being used in its broadest sense as being the lessons taught by experience. There had of late been several railway accidents, and there was an impression that this was due to the want of punctuality. That, however, was not the case; and the chief cause was the great rush of traffic at this season of the year. The fact was that a first-class railway carriage, as Mr. Bright once said, was one of the safest places in the world. In the three years ending 1849, the number of passengers killed on railways from causes beyond their own control was 12, or 1 in 4,782,188. In 1871 the number so killed was also 12; but the number of passengers had so increased that this was only 1 in 31,250,000. More were killed by their own carelessness than from the defects of the railway companies. The average in the three years ending 1871, was 18 of that class against 17 of the other. The safety of railway travelling was shewn by the fact that 3d. would insure £1,000, which put the chance at 80,000 to 1. Statistics, however, proved that it was really 7,000,000 to 1. Taking the average of each railway journey at 10 miles, one passenger was killed for every 312,000,000 miles travelled. Therefore, if a person were to travel 10 hours a day at 30 miles an hour, 365 days in the year, he ought to travel 2,850 years before his time came. Dealing with his second proposition, Mr. Preece proceeded to point out the causes upon which thi

freedom from accidents depended, and the dormant dangers thus implied. A close analysis of 138 accidents shewed that there were due to defective permanent way, 18 per cent; to defective rolling stock, 18 per cent; to defective signals, 18 per cent; and to defects in the human machinery, 41 per cent. In the next place the lecturer proceeded to trace the progress and characteristics of the signal system—the language or telegraphic system between signalman and driver to secure motion and safety. In the early days of railway travelling, when trains were few, no necessity existed for signals of any sort; but it was quickly found, as the traffic grew, that it was necessary to indicate to the driver when danger was before him; and home, or station signals were introduced. These consisted on the broad gauge lines, by day, of a bar and a disc, the former meaning stop, and the latter all right. Singularly enough, on the narrow gauge lines the meaning of these signals was reversed. By night lamps were used, and their signification was the same all over the country. As said the railway rhyme—

> "White means right, red means wrong,
> Green means slowly go along."

The semaphore system had the great advantage that it possessed two distinct signals which were similar seen from every point, whilst the disc signal might from certain points of view be misunderstood. The home signals were associated with distant signals, some thousand yards or more from the station, and in addition, hand lamps, flags, whistles, and head and tail lights to trains, were all pressed into the signalling service. Of the railway accidents, fifty-six per cent. were collisions. The method of working the railways in the earlier days of the system was to keep the trains apart from each other by an interval of time. After a train passed a point no other train was allowed to pass for five minutes, and then the caution signal was shewn for five minutes more. Now, if trains always ran at the same speed, if signals were always right and were always attended to, and if no unforeseen casualties happened, such as the breaking down of a train between two signal points, this system would secure safety. The conditions were notoriously quite the reverse. If, however, trains on the same line were always kept a certain *distance*, instead of a certain *time* apart, no collisions could happen. This, in practice, was the block system; and here it was telegraphy came to the aid of the railway manager. Mr. Preece explained in the clearest manner how electricity produced magnetism, magnetism attraction, and how by this attraction the various forms of telegraphic apparatus were worked. The block system in all its details was then illustrated, and the beautiful manner in which electricity lends itself to everything that could be required of it—even to indicating automatically the condition of distant signals, the fact whether a lamp a mile off was in or out—shewn by experiment. Reference was likewise made to the question of electric communication between passenger and guard; and the lecture brought to a close by an eloquent peroration. It was listened to most attentively, and was singularly interesting and able.

Obituary.

SIR FRANCIS RONALDS, F.R.S.

It is with regret we notice the death of Sir Francis Ronalds, which occurred on August 9th, at the age of 85. In view of the great service rendered by him to science and especially to telegraphy, of which he was an early pioneer, a lengthened notice of his life and services will appear in the next number. He was a member of the Society from its very commencement.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. II. 1873. No. 5.

The Fifteenth Ordinary General Meeting was held on Wednesday, the 26th March, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

The following Paper was read "ON A METHOD OF TESTING SHORT LENGTHS OF HIGHLY INSULATED WIRE IN SUBMARINE CABLES," by PROFESSOR FLEEMING JENKIN, F.R.S.

THE object of the present paper is to describe a method of testing the insulation of submarine cables with Sir William Thomson's Quadrant Electrometer.

The plan is a simple and convenient modification made by Sir William on the arrangement he has previously used, and the system has been introduced by him, in concert with myself, for the tests of the Great Western Telegraph Company's Cables, and more lately for the cable of the Platino-Braziliera Telegraph Company. The method has been found so convenient in practice that, although it contains no very striking novelty, I hope that it may interest the Society.

It is well known that the insulation resistance of a cable can be measured by observing on the Electrometer the gradual fall of potential, occurring when the conductor, after having been charged by a Voltaic battery, has been disconnected from the battery and left in connection with one quadrant of the Electrometer, the other quadrant being to earth.

Let R be the insulation resistance in megohms, S the capacity of the conductor in microfarads; P the potential to which the battery employed charges the conductor (in any units); p the potential to which the conductor falls in any time t expressed in seconds; then it is well known that—

$$R = \frac{\cdot 4343\ t}{S \log \frac{P}{p}},$$

and in this equation we may write for P and p, the simple deflections of the quadrant electrometer. This method has the very serious defect, that with high insulation resistances, the differences between P and p are extremely small, unless t be made inconveniently great, and even when the fall of potential occurs somewhat rapidly, the accuracy of the test is not very satisfactory, being limited by the accuracy with which the difference of the deflections can be read, this difference being necessarily only a fraction of the whole length of the scale.

The modification now to be described has the effect of increasing the delicacy and accuracy of the test. This is effected by virtually prolonging the scale so that the deflections are not counted from any visible mark on the scale, but from what may be called the *inferred zero*, an imaginary point which may be many thousand divisions to the right or left of the actual scale. The whole potentials P and p are then represented by thousands of divisions instead of hundreds, the fall is measured in hundreds instead of in tens, and by a proper choice of the inferred zero, the deflections showing the fall may be made for each class of cable to extend over nearly the whole length of the actual scale.

One quadrant is permanently connected with one pole of a battery, the other pole of which is to earth.

The second quadrant is connected with the conductor of the cable to be tested, and is charged along with the conductor to the same potential as the first quadrant by a short contact with the same pole of the same battery. When this short contact is first broken, the two quadrants are at the same potential. The potential of the quadrant connected with the cable will fall, in consequence of the leakage through the insulating sheath of the cable, but the potential of the second quadrant will remain unchanged during the experi-

ment; the spot of light will move from its first undeflected position, which I will term the actual zero, towards one end of the scale. Let us assume the sensibility of the electrometer to be such that one cell would give a deflection of 100 divisions, and that we charge the cable with 100 cells; then as soon as the potential has fallen 1 per cent. we shall observe a deflection of 100 divisions on the scale; by the time the potential has fallen 4 per cent. we shall have a deflection of 400 divisions. What is termed the "inferred zero" is the final reading which we should obtain if the cable and its quadrant were wholly discharged, while the other quadrant remained fully charged to the maximum potential. Clearly this, in our example, would be 10,000; the inferred zero cannot be directly observed, but is obtained with great accuracy by using resistance-coils to subdivide the potential of the battery. The two poles of the battery are permanently joined through a high resistance with convenient subdivisions; the electrometer reading given by any convenient fraction of the maximum potential is thus obtained in a well-known manner, and the inferred zero is given by a simple multiplication.

The battery, C Z (frequently 100 cells), is connected direct to the cross-bars of an ordinary reversing key, A, the springs of which are joined to the extreme ends of the slide, G H, G being also connected to earth.

An ordinary set of resistance coils may be used instead of the slide.

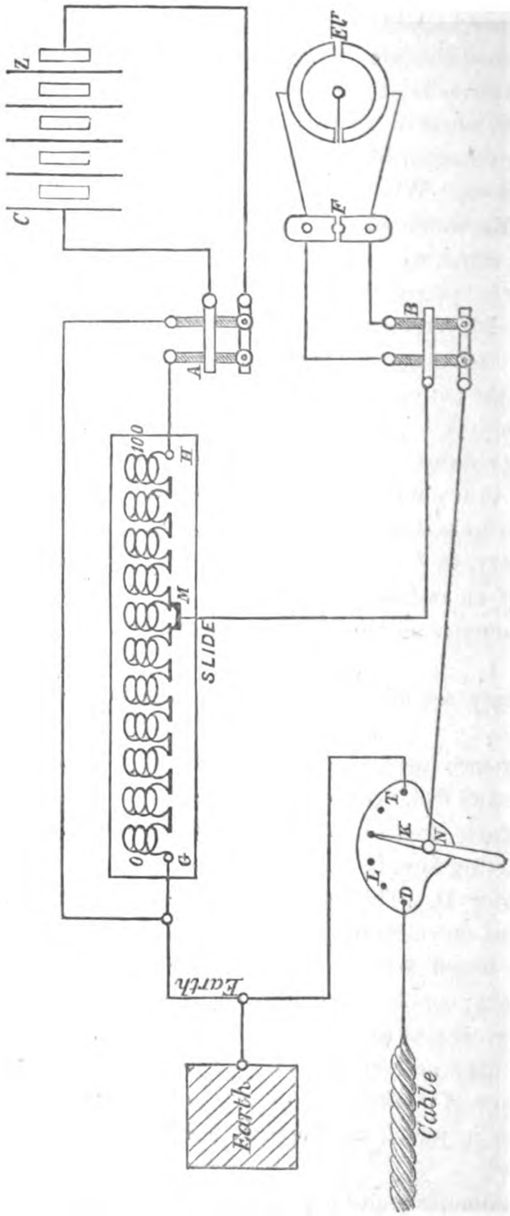
The resistance used should be sufficiently great not to diminish sensibly the full difference of potentials that the battery can produce.

The moveable contact of the slide is connected to one bar of the second reversing key, B, the other cross-bar being connected at N to the distributor D. This consists of six or eight brass terminals, L, well insulated on vulcanite pillars, arranged in the arc of a circle. The cables to be tested are connected to these terminals, which are tipped with platinum; contact can be made with any of them by the moveable arm K, which is also tipped with platinum. The object of the distributor is to allow several cables to be tested simultaneously.

The springs of the key, B, are joined to a short circuiting piece, F, which again is joined to the two well insulated electrodes of the electrometer.

The electrometer scales are divided into half millimetres, and numbered from zero to one thousand, beginning at the left hand side. The

The diagram shows the connections actually employed.



actual zero of the electrometer is arranged to be at or near the zero of the scale, and the fall of potential is observed always in the same direction, the readings increasing from zero as the charge in the cable diminishes. When the battery is reversed by the key A the second reversing key B reverses the electrometer also. The inferred zero being the limit to which the readings on the electrometer tend as the cable discharges is equal to the value of the battery in scale divisions, if the electrometer starts from zero on the scale. To find the inferred zero the moveable contact M is placed so that there is the tenth of the whole resistance of the slide between G and M. One of the springs of the key A is depressed, and also the corresponding one of key B. The short circuiting plug is removed, and the distributor turned on to the last terminal T, which is in connexion with the earth. The deflection thus obtained multiplied by ten gives the distance in half millimetres of the *inferred* zero from the *actual* zero reading. The inferred zero having been found, the arm K of the distributor is removed from the terminal T, the contact M moved up to H, and the short circuiting plug inserted. The cable to be tested is attached to one of the terminals L, and is now connected with the battery by the arm K, which will be seen by the diagram to be in connexion with H (and, consequently, C or Z) through the short circulating plug at F. This battery contact is maintained sufficiently long to establish electrical equilibrium throughout the cable, fifteen seconds being enough in general. After this time the plug in the short circuiting piece F is removed, leaving the cable in connexion, through the distributor and key B, with one electrode of the electrometer.

The potential of the cable now begins to fall while the electrode of the electrometer not in connection with the cable is maintained at the full potential of the battery by connexion with H; hence the difference between the electrometer reading (at any time) and the inferred zero gives the potential of the cable in scale divisions at that time.

The difference of the logarithms of any two of these values of the potential of the cable gives a definite measure of the quality of the insulating material, independently of the length of the cable, and the actual insulation is given by the above formula.

To find the insulation resistance after two minutes' electrification, readings are taken at 1′ 45″ and 2′ 15″ after the battery is first applied to the cable. These readings are subtracted from the inferred zero, and the logarithm of the differences taken.

A constant, $\frac{\cdot 4343\, t}{S}$, divided by the difference of the two logarithms thus found, gives the insulation resistance in megohms.

Example; Hooper's Material at 75° F—

Actual Electrometer zero	... ...	100
Reading with tenth B ...	... ...	650
Difference ...	... ...	550
Difference Multipled by ten	... ...	5500
Electrometer Zero ...	... ...	100
Inferred Zero	... ...	5600

Time.	Reading.	Diff. from Inferred Zero.	Log.	Diff. of Log.
1′ 45″	354	5246	71983	
2′ 15″	395	5205	71642	00341

$$\text{Constant} = \frac{30 \times \cdot 4343}{\cdot 43} = 30{\cdot}3.$$

$$R = \frac{30{\cdot}3}{\cdot 00341} = 8890 \text{ megohms per knot.}$$

The advantages of this mode of testing, as compared with the common electrometer, are, increased delicacy and accuracy, as has been explained already.

The delicacy of the test may be increased *ad libitum*, by increasing the Electro-motive force of the battery, the sensitiveness of the instrument, and t the length of time.

When compared with any galvanometric method it has other advantages. The absence of any magnet in the electrometer allows the test to be made while machinery is in motion. Currents which from various causes are induced in the cable, showing irregular deflections on the galvanometer, are unperceived on the electrometer. Short coils of high insulation resistance, which would show no sensible leakage on the galvanometer, are easily tested by the new system, and last but not least, several coils or cables can be tested at one time with a saving of labour. The distributor is used for this purpose. One coil is attached to each terminal; each coil is electrified in succession, and then detached from the quadrant until a few seconds before the reading is required. In this way long tests of electrification can be made on say five coils in one-fifth of the time required for

galvanometer tests. At Millwall we have in this manner tested three cables at one time, obtaining a complete separate series of tests for each cable. The labour of reducing the tests to resistance per knot is small, and the inferred zero of the electrometer is much less liable to change than the constant of a galvanometer.

I am indebted to Mr. Wm. F. King for the description of the details of the arrangement actually in use at Millwall. Mr. Bottomley at Millwall finds that (testing two cables at once) there is a saving of 38 per cent. in the time required for the tests of the tanks.

The Chairman having invited discussion on the above paper, and Mr. King having expressed his readiness to answer enquiries,

Mr. C. F. Varley said he had not many observations to make. The paper was a short one, but important. The title of it was hardly correct, as it was described as a method of testing short lengths of highly insulated wire, whereas it was equally applicable to the testing of long lengths. One of the great conveniences experienced in the use of the electrometer instead of the galvanometer was, that it was impossible for the former to be influenced by a discharge through it, as occurred frequently with the galvanometer. He had had long experience of this mode of testing, and found that in all senses the test by fall of potential was superior to ordinary testing by means of measuring the current through the galvanometer. In the year 1866 the Telegraph Construction and Maintenance Company engaged Sir W. Thomson and himself to draw up the system of testing for the 1866 Atlantic Cable, and that system had been used, with little modifications, ever since on all the large cables. One of the methods of testing, and the original plan which was carried out with the '66 cable, was to use the electrometer, not in the form shown by Professor Fleeming Jenkin, but in a somewhat analagous form, and he would describe that as being useful in many cases, as it got rid of one of the slight errors which he would point out, which occasionally come in in this method. The battery was charged precisely the same as that on board the Great Eastern, and everything was done to make the battery at Valentia equivalent to that on board the ship, except that it was 120 cells at Valentia and only 100 cells on board the Great Eastern, the object being that, should there be an earth current at any time on the line acting in

conjunction with the battery on the Great Eastern, it should have power to measure the 100 cells of battery on the Great Eastern, plus the earth current when it was acting in the same direction. This was the ordinary Thomson slide or potential divider; this the sub-divider which he (Mr. Varley) invented, by which each of the coils here were reduced so as to give fractions of each coil. In the potential divider there are 101 coils: the sub-divider has 100 coils, the resistance of which are equal to two coils in the potential divider; this reduces the resistance of the 101 coils from 101,000 ohms. to 100,000 ohms., so that the battery potential is divided to 10,000th parts, so that by means of the two slides they could sub-divide the potential to $\frac{1}{10000}$ part of 100. He ought to tell them this electrometer was so sensitive that a single cell gave over 500 divisions of deflection. Supposing the potential were 99.9, then these two slides were put up till one was at 99 and the other at 90, and then immediately the electrometer came to zero. By these means the same result was obtained as by Professor Jenkin's plan, but with this difference—that if there were any irregularity in the divisions of the electrometer, they would very likely find 12 divisions at one part of the scale would equal one cell, and perhaps only $9\frac{1}{2}$ at another part. If the scale was noted carefully beforehand, then no error would arise by Jenkin's method; and as that was the only point to which special attention required to be directed, so far as he saw from the paper, in testing very short lengths of wire, the galvanometer, if used for the purpose, entirely failed; whereas, by noting the fall of the potential by the time of the electrometer, the measure was as correct for short lengths as for long ones. Whenever they were working with very short lengths—1 or 2 feet only—then another difficulty arose, and that was they must not work in a room in which there are flames burning; for it would be found the flame discharged through the air some of this charge, and then working with a small piece of wire they got a lower insulation than the truth. To meet that, when he had had short lengths to deal with, he tested two lengths, one longer than the other, and the difference between the two indicated the error. The absence of the magnet in the electrometer to which Professor Jenkin called attention was a very important one, for those not practically engaged in testing cables would scarcely be aware of the inconvenience of the magnetism of moving machinery, which kept the galvanometer in such a state of oscillation as to prevent

accurate reading, and it was necessary to postpone accurate testing till the dinner hour, or till the working was stopped for the night. With regard to testing simply by the fall of potential, undoubtedly the method of Professor Jenkin was the nicest yet introduced, and it had been used in the French Atlantic Cable for two years. The cable was kept negative to the ocean by means of a special battery. [Mr. Varley illustrated this on the board, and explained the action of battery, &c.] Being connected in that way, this battery would keep the cable always negative to the ocean; but inasmuch as the resistance to the fault was tolerably constant, and as this resistance was great, it followed that a small current was flowing in there [pointing] which kept the cable negative to the ocean, &c. The moment this negative signal charge was thrown in, the cable became more negative. If, on the other hand a positive signal charge was thrown in the cable became less negative. They kept the electrometer in circuit there [pointing]. All that was done in taking the morning test was to disconnect the protecting battery and note the fall of potential. These were reduced to actual insulation resistance, and this method for two years had worked admirably. The cable was never entirely discharged, and he mentioned it in confirmation of what he had said, that it was the best method they could adopt for long cables which were perfect or nearly so.

Mr. Phillips said the paper spoke of charging the cable for 15 seconds. He would be glad to know what length that referred to. He had taken these tests by galvanometer discharges, and the chief difficulty he had was the time of charging the cable up. In the case of a short cable, charging it for one minute and then leaving it for one minute, the value given by the formula before them now nearly agreed to a second minute's value of ordinary reading. What he wished to know was, did the charging for 15 seconds give values which agreed with the first minute's value obtained by the galvanometer. If so, on what lengths would it be safe to use that time?

Mr. King replied, that 15 seconds were included in the time of insulation. The cable was charged first for 15 seconds; it was then insulated for 1′ 45″ after the time the cable was first charged or, 1′ 30″ after the time the battery was removed.

Mr. Phillips: Is that supposed to give the value of the second minute reading?

Mr. King: This test is altogether the second minute. The test of one minute has to be subtracted from this number; the first reading being 45″ after the battery is first applied, and the second reading 30″ after it is removed. With reference to what Mr. Varley says about the scale values of one cell being different at different parts of the scale, I have found it possible, by proper adjustment of the Electrometer, and using a circular scale, to get the reading more correctly proportional to what Mr. Varley has given, viz: 2 per cent.

Mr. Varley: I give them not as an accurate representation.

Mr. G. K. Winter remarked the method they had heard described this evening, whereby the sensitiveness of the electrometer test for insulation might be so largely increased was certainly a great step, but, unfortunately, it left the chief objection unaffected. He spoke of the impossibility, in the present state of their knowledge, of separating in the electrometer test the apparent leakage due to absorption, from the true leakage due to defective insulation. With the galvanometer deflection method this was comparatively easy, for having, in the preliminary testing of the cable determined the nature of the curve of absorption, it was always possible from the apparent resistance at the end of—say three minutes, to calculate what would be the ultimate resistance. Now, he knew of no direct method of determining the curve of absorption of a charge after the battery had been disconnected. He thought it was not impossible by indirect methods and calculations; but until such had been applied, the increase of sensitiveness of the electrometer test, multiplied the erroneous as well as true reading, and at present they could not separate the two. The great difficulty in the galvanometer method consisted in the irregular deflections caused by changes in the potential of the earth, and if the electrometer was, as Professor Jenkin said, unaffected by these, it was a great deal in its favour. Had this, however, been sufficiently proved on submerged cables?

Mr. W. E. Ayrton said he did not clearly understand why the readings should be taken at such small intervals. He could understand why they should be taken at 1′ 45″ and 2′ 15″ with an interval of 30″, as a difference of 10″ in the scale made the greater error, the smaller the difference was between the two readings. It might appear that more accurate calculation could be made if the observation were made directly the battery was removed—that was 15″, then in the

two minutes it would make a difference in the two readings, if this were done and the calculations in the formula were equal, the resistance of time would not be the same as that obtained by the galvanometer after two minutes electrification, but perhaps equivalent to the resistance observed by the galvanometer somewhere about one minute or between one minute and two minutes, but not two minutes. In order to obtain the same insulation as was derived from the galvanometer test after two minutes electrification it was necessary to take readings. There might be less interval of time, that was why readings were taken just before and just after two minutes. In the same way, calculating insulation after five minutes electrification, the readings were taken, not at 5′ 0″, but at 4′ 45″ and 5′ 15″. That was just before and just after the time it was required to find the insulation. The advantage of the electrometer system was, by the use of a replenisher it was possible to make electrometer readings always mean exactly the same thing, but on moving a mass of iron in the neighbourhood it was impossible to make a constant of any galvanometer exactly the same from day to day, or during the whole of one day, so that it was difficult to use a table for calculating the resistance of a cable from the deflection, that was, a table which would give at once resistance of deflection, but by the use of the replenisher a table could be used which gave the resistance of the cable at once from the difference of logarithm of time. He might mention it was possible with the system of Professor Jenkin, in seven minutes, to calculate in megohms the resistances of two cables, each after two minutes electrification and after five minutes electrification. They might have the four resistances of four cables after two minutes electrification and after five minutes electrification.

The CHAIRMAN said, he had with great pleasure to propose a vote of thanks to Professor Jenkin, for the very practical paper with which he had favoured them. The author had said, very modestly, there was no striking novelty in his system. Essentially the system had been used considerably, but the value of the paper was calling the attention of members, and inducing others to point out the merits of a system of measuring by potential and electrometer, as compared with what he did not call the old method, but the ordinary system of measuring by resistance. That there were advantages in measuring by potential, which must force themselves upon electricians. An electrometer was not a nice instrument to carry about for use, but

the luxury of using it when at hand was very great. In measuring the resistance of a great length of cable, every one who had done so must have been troubled by the changes of the potential of the battery, or the potential of the earth current. A change of the hundredth part of a cell threw the needle over wrong for many minutes. He tested the 1865 and 1866 cables by the method described by Mr. Varley, though the earth current varied so much that it was not possible to take tests with a galvanometer, he might say the importance of measuring by potential was great, and there was another way of doing it, that was by the use of the standard cells which he described on a former occasion, and having an electrometer to take the place of the slide when one had not a slide at hand. It was also possible to use the potential method of testing by the use of condensers and the ordinary galvanometer; but his advice to all was to possess themselves of an electrometer, even if it were one of the cheaper form, as soon as possible, for when they got into the habit of using it he was sure they would never go away from it. The Chairman concluded by moving a vote of thanks to Professor Fleeming Jenkin for his paper, which was unanimously adopted.

Mr. Ayrton then read the following paper:—

ON SOME POINTS IN CONNECTION WITH THE INDIAN TELEGRAPHS.

By W. E. Ayrton.

This paper is not intended to be at all a complete account of the Indian Telegraph, for to enter at length into the details of this or any other Telegraphic Administration would require a book rather than a single paper. I have, therefore, only ventured to select one or two points that I thought might interest those present.

It may be stated in commencement that throughout the greater portion of India there exist two distinct lines of telegraph, one in possession of Government, the other in the hands of the various railway companies. These two sets of lines are almost entirely distinct, being under separate management, and worked by different sets of employés. The rules, however, relating to tariff and to the reception of messages, are the same for both systems, and a message may be conveyed partly over the Government and partly over the Railway lines, at a single cost to the sender. With this sole exception, the two sets of lines differ so

widely that I wish it to be understood that anything I say this evening refers only to the Government, and not to the Railway Telegraphs.

The application of the laws of Electricity to the practical purposes of testing, and to the determination of the best form of instruments, and the most suitable arrangement of batteries, having been somewhat developed in India I have entered rather at length into this subject. Many other branches, however, equally important, such as the administration and discipline, the expenditure and receipts, and the construction and conservancy of the lines, I have been compelled to leave untouched. Some information on these points, however, may be given us, I hope, by a more able member of the Indian Telegraph, and by one whose much greater familiarity with this portion of the subject will aid him in doing it more justice than lies in my power.

TESTING OF LINES.

Regular testing of the lines was first introduced into India about the year 1868, and since that time the advantage of executing these tests has become more and more fully appreciated. Every important line is now tested two or three times a week in the following manner.

With a Wheatstone's Bridge, or a Differential Galvanometer, observations are made with both positive and negative currents of the resistance of the line, under the three following conditions:—

1st. When put to earth through the relay at the other end.

2nd. When put direct to earth (that is, relay short-circuited).

3rd. When insulated at the distant end.

From the six values thus obtained the following are calculated by equations* suited to the form of testing instrument employed.

---

*
$$e = \frac{N_1 - P_1}{P_1 + N_1 + l_1} \; E$$
$$= \frac{N_2 - P_2}{P_2 + N_2 + l_2} \; E \text{ also}$$
$$A = \frac{m_1}{l_1} \times \frac{l_1 (P_1 + N_1) + 2 P_1 N_1}{P_1 + N_1 + 2 l_1}$$
$$B = \frac{m_2}{l_2} \times \frac{l_2 (P_2 + N_2 + 2 P_2 N_2}{P_2 + N_2 + 2 l_2}$$
$$C = \frac{m_3}{l_3} \times \frac{P_3 + N_3}{2}$$

where e is the electromotive force of the Natural Line Current, E that of the testing battery—P_1, N_1; P_2, N_2; P_3, N_3 the values obtained with the Positive and Negative currents respectively in the three tests, and l_1, m_1; $l_2\ m_2$; l_3, m_3 the three sets of branch resistances l_1, l_2, l_3 being opposite in the Bridge to the unknown resistances.

As the difference between the positive and negative values in the Insulation test is frequently due more to chemical action than to a natural current, the arithmetic mean of the two readings obtained, when each current is kept on for the same time, gives, perhaps, the best approximation to the true value.

1st. The electromotive force of the natural line current in terms of that of the testing battery, usually 40 cells.

2nd. What the apparent wire resistance (A) of the line, including the relay at the other end would be, were there no natural line current.

3rd. What the apparent wire resistance (B) of the line, excluding the relay at the other end, would be on the same supposition.

4th. What the apparent insulation resistance (C) would be also on the same supposition. The object of determining (A) at all will be seen hereafter.

If the natural line current be large, so that there is a considerable difference between the observed values obtained with the positive and negative currents, then the real values differ much from the arithmetic mean of the observed values.

The equations from which (A), (B), and (C) are calculated are determined on the supposition that the difference between the positive and negative readings is due to a natural current uniform throughout the whole line. This is, of course, frequently not the case, as when, for instance, the natural current flows towards each end of the line from about the centre, the connection between the line at that point and the earth being formed by dirt or moisture accumulated on the insulators. In such a case, however, it is extremely difficult to ascertain the distribution of the natural potential at different points of the line, and in addition the equations for determining the true means from the positive and negative readings become much more complicated, and unsuited for practical use.

The next point to consider is, that if the line be long or badly insulated, then (A), (B), and (C) will not represent respectively the real values of the resistance of the line including the relay, of the line excluding the relay, and of the insulation, because in each case a complicated circuit has been tested, consisting partly of wire and partly of insulation resistance. In fact on such a line (A), (B), and (C) are often nearly equal to one another, (A) and (B) being less, and (C) greater than the real value. For determining (W) and (I), the real values of the resistance of the wire and of the insulators independently of one another, the following two equations are used :—

$$W = 2 \left(C - \sqrt{(C - B)} \right)$$
$$I = \sqrt{C (C - B)}$$

which equations are calculated on the supposition that the resultant fault is electrically at the centre of the line, by this I mean on the supposition that the real wire resistance of the line is the same from each end up to the point at which a single leakage could be substituted for all the different leakages without altering the electrical condition of the line. This, of course, is not always the case, when, for instance, one end of the line is worse insulated than the other. The same equations for determining from (B) and (C) the real values (W) and (I) are, however, for simplicity always used, and their accuracy in any particular case tested as follows:—If the resultant fault be really at the electrical centre of the line, and consequently (W) and (I) be the true values, then the resistance of the relay at the other end should (as can easily be shown) be given by the fraction:—

$$\frac{C\,(A - B)}{C - A}$$

what this resistance really is can be ascertained by enquiry, for the resistance of every relay in use in India is stamped on the instrument. If it be found that this fraction gives a value $\left\{\begin{array}{c}\text{larger}\\ \text{smaller}\end{array}\right\}$ than the true resistance of the relay, than it is known (as may easily be proved by a simple calculation) that the resultant fault is $\left\{\begin{array}{l}\text{farther from}\\ \text{nearer to}\end{array}\right\}$ the testing station than the distant station, and also that the equations used for determining (W) and (I) have given values somewhat $\left\{\begin{array}{c}\text{larger}\\ \text{smaller}\end{array}\right\}$ than the real wire and insulation resistance. In this way the position of the resultant fault is roughly determined, from which we know the relative insulation of the two halves of the line, and in the case where the resultant fault is at about the electrical centre of the line, then the real wire resistance, and the real resistance of the insulators quite independent of one another and of all natural line currents are also found, no matter how badly the line be insulated, or how strong the natural line currents be, provided they be only constant during each pair of tests. In the case where the resultant fault is not at the electrical centre of the line, then values differing somewhat from the real wire resistance and the real resistance of the insulators are determined, but whether they are larger or smaller than the true values is also known.

As an example of the importance of their consideration, I will take the following example:—

Let the line under test be 500 miles long, and let the relay at the other end be known to have 2,000 ohms resistance;

$$\text{Let} \quad A = 3884{\cdot}6$$
$$B = 3611$$
$$C = 4500$$

Now if these were taken as the real values without any correction being applied, we should say that the wire resistance per mile was $\frac{3611}{500} = 7{\cdot}26$ ohms, and that the insulation per mile was $500 \times 4{,}500$, or about 2·2 megohms.

Now from the two equations (1) and (2) we have

$$W = 2\left(4{,}500 - \sqrt{4{,}500 \times 889}\right)$$
$$= 5{,}000$$
$$\text{And} \quad I = \sqrt{4{,}500 \times 889}$$
$$= 2{,}000$$
$$\text{And} \quad \frac{C\,(A - B)}{C - A} = 2{,}000 \text{ about.}$$

we know, therefore, that the resultant fault is at the centre of the line, or that the leakages in the two halves of the line are similarly distributed with reference to the centre; consequently the values of (W) and (I) given by equations (1) and (2) are correct. The real wire resistance per mile is therefore $\frac{5000}{500}$ or 10 ohms, and the real insulation per mile $500 \times 2{,}000$ or 1 megohm. The values, therefore, obtained for these without applying the correction, are respectively 40 per cent. too small, and 54 per cent. too large.

In all cases, of course, the exact position of the resultant fault, whether it be at the electrical centre of the line or not, could be determined, and afterwards the true values of the wire and insulation resistance; the exact calculation, however, becomes exceedingly complicated, except where the resultant fault is at the electrical centre of the line.

It is, of course, very difficult to localise faults accurately on a line in which the normal absolute insulation is not much greater than the absolute wire resistance, since the worse insulated a line is in its usual state the more difficult it is to localise any extra leakage. Theoretically correct results can be obtained for "earth faults" by using the "centre of gravity" method. To do this, however, it is necessary to know what would be the magnitude and position of the resultant fault, if the extra fault, the position of which it is attempted to localise, did not exist. Now it is almost impossible to ascertain this

in practice, since the magnitude of the resultant fault, which is the absolute insulation of the line, varies perpetually during the day and night, even although the line be in good order, and it is almost impossible to predict accurately what it would be at any particular time. A simple correction, however, can be applied to the results obtained by the ordinary tests for earth faults and contacts, if it be remembered that the effect of the general leakage of the line is to make the fault apparently farther from the testing station than it really is if it be in the near half of the line, and nearer to the testing station than it really is if it be in the distant half of the line. Taking into account this consideration, faults are usually localised in India to within one or two per cent. of their real distances. A great deal, however, of course, depends on the individual skill of the tester.

The routine for testing is as follows. The testing station commences by calling the name of the nearest station in circuit on the particular line it wishes to test, until this station replies with its name. The testing station next signals the word "testing." The signaller at the distant instrument on receipt of this at once calls the telegraph master, who signals back his initials, and takes charge himself of the instrument until the testing is over. The testing station now signals "circuit," on which the telegraph master leaves the instrument and line alone until he hears the name of his station called by the testing station. After the word "circuit" has been signalled, the resistance of the line, including the relay at the other end, is found. Next the testing station signals "conductor x minutes," to which the telegraph master replies with the temperature (dry and wet bulb) at his office, and at once puts the line direct to earth, short circuiting the relay for the specified time. The resistance of the line excluding the relay at the other end, is then found. The testing station next signals "Insulation x minutes," to which the telegraph master replies with the state of the weather, and at once insulates the line for the time specified. No conversation of any kind beyond the above is allowed, unless of course the testing station asks any particular questions. By following out this routine rigidly the tests are performed with certainty and dispatch. When it is required to test a line beyond an office usually in circuit, this office receives orders "testing join a certain line x minutes" in accordance with which that office short circuits its galvanoscope, &c., and joins over the particular line for the time specified, exactly as if that line did not come into the office at all.

In the case of faults, to determine the position of which I may mention tests are always at once made day or night, the testing station signals its orders as insulation, conductor, loop, &c., in the ordinary way, provided communication on all lines in the faulty section be not interrupted. When, however, a post, for instance, has been blown down and all the lines have tumbled into water, so that it is impossible to communicate on any line, then, at the commencement of the next hour (Madras time), three or four o'clock, or whatever it may be after the fault has occurred, the office at the end of the interrupted section insulates all lines from the hour to 15 minutes past the hour, then puts them to earth through the relay for the next 15 minutes, then puts them direct to earth for the third 15 minutes, and for the last 15 minutes of the hour loops the lines in pairs previously settled in printed instructions supplied to each office. This routine is repeated every hour until communication is restored. In this way the testing station knows at any moment exactly what is being done with the ends of the lines at the next office beyond the interruption, and so is frequently able to localise faults, although communication on all lines has been stopped.

After the fault has been localised, a telegram is sent, if possible, to the office nearest the fault, telling them where to send a man, and what sort of fault to look for. In this way, one man going from the nearest office is able to remove the cause of interruption, whereas, before the systematic introduction of testing in India, two men travelled along the line, one from each of the stations at the end of the interrupted section, until one or other found the fault, and since these men could not travel by night for fear of passing the interruption, whereas now they may start at once and travel by the most expeditious means up to the spot at which the testing shows the fault to be, interruptions are not only removed at less expense, but in addition in a much shorter time than formerly.

I may mention that where faults have occurred during the night not many miles from the testing station, and on lines whose normal insulation was good, I have on a few occasions, by localising the position of the faults very carefully, succeeded in having them removed during the night by men sent out with lanterns, so that scarcely any interruption whatever to the traffic has been caused.

All tests, ordinary and fault, are carefully worked out and tabulated at each testing station, and reports are sent weekly to the office of

the Electrical Superintendent in Calcutta for re-calculation and compilation in bi-yearly reports for submission to the Director-General. In this way the electrical history of every line is carefully recorded, from which the relative qualities of the different lines are derived, so that the best lines can be portioned out for working direct the longest distances.

Although testing for faults is of immense utility, the regular testing, which has now been carried on for about four years, under the Electrical Superintendent, Mr. Schwendler, has produced even more valuable results. By it we have learnt two very important facts; one, that the earth plates in many offices had a high resistance, and were intensely polarised; the second, that the insulation resistances of sections varies enormously under precisely the same climatic influences, it being on some sections over 300 megohms a mile (the limit of the measuring power of the testing apparatus employed in the particular instances), while in other sections it is considerably less than one megohm per mile. The first evil has been remedied by using large copper earth plates with leading wires insulated from the ground; the advantage gained by insulating these latter I will consider when we come to testing earth plates. The cause of the badly insulated sections is found to have been produced by solitary insulators scattered here and there over these sections, and which are apparently quite good to the eye, but are found to possess perhaps less than the five hundred thousandth part of the resistance of the general run of insulators of the same pattern, made by the same maker, and under precisely the same conditions as regards dew, &c., and when tested in exactly the same manner. How serious is the injury done to lines by the presence of a few bad insulators will appear from the following considerations. If a line five hundred miles long be composed entirely of good insulators, each having a resistance of 500,000 megohms (rather a low resistance, as will presently be seen, if the edges of the porcelain cups be clean and dry, as of course they are during the hot dry weather in India), and if *twenty* insulators per mile be used, then the absolute insulation of the line will be 50 megohms. Whereas, if *only one* per cent, of bad insulators, each having *half* a megohm resistance. be scattered uniformly over the line, then the absolute insulation will be reduced to only about five thousand ohms. The line in the two cases will be as in figures (1 and 2) if it be composed of No. 5½ wire having a resistance of 10 ohms per mile,

and if a relay of 3,000 ohms resistance be used at the receiving station. Consequently the current leaving the sending station will, in the first case, if we neglect the resistance of the battery, and if E be its electromotive force, be about equal to—

Fig I.

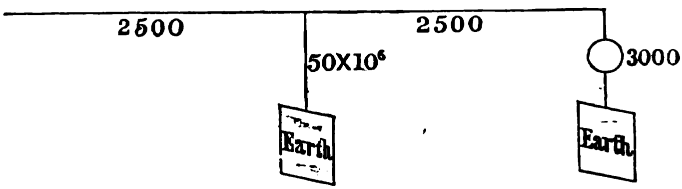
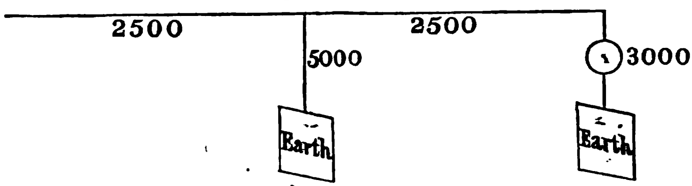


Fig II.



$$\frac{E}{2500 + \dfrac{50 + 10^6\,(2500 + 3000)}{50 + 10^6 + 2500 + 3000}}$$

$$= \frac{E}{8000} \text{ nearly}$$

In the second case, the current leaving the sending station, if the same battery be employed, will be about equal to

$$\frac{E}{2500 + \dfrac{2000\,(2500 + 3000)}{5000 + 2500 + 3000)}}$$

$$= \frac{E}{5000} \text{ nearly}$$

therefore with the bad insulators, the current leaving the sending station will be $\frac{8}{5}$ of what it would be if all the insulators were good. Consequently the introduction of these bad insulators will increase the consumption of battery materials by about 60 per cent. Now, let us consider the current arriving at the receiving station. Where

all the insulators are good, this will be about equal to

$$\frac{50 \times 10^6}{50 + 10^6 + 2500 + 3000} \times \frac{E}{8000}$$

$$= \frac{E}{8000} \text{ nearly}$$

In fact, scarcely any of the current will be lost *en route* in this case. When the one per cent. of bad insulators are introduced, the current arriving at the receiving station will be about

$$\frac{5000}{5000 + 2500 + 3000} \times \frac{E}{5000}$$

$$= \frac{E}{10000} \text{ nearly}$$

therefore the current arriving at the receiving station will, with the bad insulators, be about $\frac{4}{5}$ of what it would be if all the insulators were good. We may, therefore, say that in the particular instance we have been considering, the introduction of this one per cent. of bad insulators increases the consumption of battery materials by 60 per cent., and diminishes the received, or effective current, by 20 per cent.

From some thousand of Robinson insulators that I lately have been testing at Messrs. Siemens, I find that the average resistance, after one minute's electrification of a good Schomberg porcelain is about four million megohms, of a good Pinder Bourne porcelain about two million megohms, and of a good Defuisseaux porcelain about five hundred thousand megohms, the insulators in all cases being inverted in pure water with water in both porcelain cups, but with the edges of the porcelain cups dry (artificially dried with hot irons, if necessary). We might, therefore, reasonably expect that a line, composed of such insulators, should have in the hot dry weather in India a minimum insulation of at the very least a thousand megohms per mile, if the insulators be not damaged. It will be seen then, in the instance previously taken, a rather low resistance was used for the good insulators, and the harm done by the insertion of bad insulators, was not made greater than it really is. These results being fully appreciated by Colonel Robinson, have induced him to establish at Calcutta, Bombay, and Madras, regular arrangements for continuous insulator testing, and now no single insulator is used in India that has not been previously tested at one of these places, in the following way. First

each finished insulator, as it arrives from Europe, is tested individually with a delicate Thomson's galvanometer to see that the resistance of the insulator, after one minute's electrification, is not below a certain standard. Next the insulators are joined together in hundreds, and the average resistance found. The importance of testing the insulators individually is only beginning to be fully appreciated.

The following plan has usually been adopted in Europe. An agreement is made between the buyer and contractor that each insulator is not to have less than a certain resistance. The insulators are only tested in hundreds, and if the hundred have more than the one hundredth part of the specified resistance per insulator, they are all passed. If, therefore, 99 of them had a resistance much above contract, and one a resistance much below contract, still the hundred might have a joint resistance much above the hundredth part of the contract resistance per insulator, and the one with low resistance would not, therefore, be detected. The buyer may answer he does not care about this, all he wants is a certain insulation per mile. Unfortunately, however, any insulator that has a resistance much below the rest will probably go on deteriorating, since the cause that originally diminished its resistance may possibly go on increasing, until the insulation per mile falls much below what is required, although it may have been considerably above when the insulators were first put up on the line.

As far as has been ascertained at present, it appears that the defects in these solitary insulators are produced by excessively minute cracks in the head of the porcelain, the part that is that fits into the iron hood, and by an accumulation in these cracks of moisture which forms a connection between the stalk and hood. These minute cracks are in many cases certainly not produced by simple mechanical injury such as insulators might receive in travelling, or when upon the line, from excessive strain caused by the wire, since a considerable per centage of some porcelain insulator cups sent out to India, and to which neither hoods or stalks had ever been fitted, were defective in the manner I have described, and had individually less than a megohm resistance.

If the solitary bad insulators be scattered at all uniformly over a line, the only way in which they can then be detected is by testing every insulator singly. Now, to carry a delicate galvanometer and a large battery from post to post for this purpose would be exceedingly

inconvenient. To avoid this Mr. Schwendler has devised a powerful magneto-electric arrangement, by which a rapid succession of reverse currents sufficiently strong to be detected with the fingers is sent through an insulator if it be very bad, or strong enough to be detected with the tongue, which is a cheap and remarkably delicate galvanoscope, if the insulator be less bad, but having a resistance below a certain amount, depending, of course, upon the power of the electro-magnetic machine.

One of these detectors was sent home from India last year, and exhibited in the exhibition where perhaps some of those present may have seen it.

At about every twenty miles, or so, on all lines, shackles are introduced by which the line is electrically broken, communication being in ordinary cases established by two spirals of thin wire soldered respectively to the line wire on each side of the shackles, and ending in eyes faced with platinum which are usually screwed together. By this arrangement a lineman can disconnect the line at about every twenty miles and communicate in either direction. This was particularly necessary before the introduction of scientific testing as it was by endeavouring to communicate first in one direction, and then in the opposite, that the man who had been sent out in case of interruption, knew on which side of him the fault lay, and even now when the faults are localised by testing, as their distance can only generally be found within one or two per cent., it is often of great use on a long line for the man to disconnect nearest to the spot localised, and see on which side of him the fault exists.

Earth Plates.

In Europe the ordinary way to make an "earth" is to use the iron gas, or water pipes, but in most places in India such pipes do not exist, so that some large piece of metal has to be buried for this purpose. A coil of iron wire, a piece of an iron post, or a copper plate have been used at different times. Now as the nature of the ground in the immediate neighbourhood of this buried piece of metal greatly affects its electrical utility, it becomes a question of great practical importance to determine in absolute units the resistance of the "earth" used in each particular case.

It might, at first sight, appear possible to obtain the resistance of the "earth" at a distant office by testing the resistances respectively

of two lines put to earth at that office, and comparing the sum of the resistances so obtained with that of the loop. There are two reasons, however, why such a system would be extremely inaccurate, first, because, if even the lines were perfectly insulated, as the resistance of an "earth" is, or should be, exceedingly small compared with that of the line, the slightest possible error made in taking the circuit resistance would produce an enormous percentage of error in the calculation of the resistance of the "earth;" secondly, as the insulation of most lines is generally not very good, the resistance of the loop is frequently greater than the sum of the resistances of the two lines put to earth at the distant station, so that if the fact of leakage were not considered the earth might be said to have a negative resistance.

The following method suggested by Mr. Schwendler, and fully developed in a paper I read before the Asiatic Society at Bengal, is at present in use in the Indian Telegraph Department, and has none of these objections :—

Select two other earths which are neither in metallic connection with one another, nor with the telegraph earth to be tested. Two iron telegraph posts, near the office, answer the purpose very well only care must be taken that there is perfect metallic contact between the leading wire and the iron post in each case. In the dry season it would be advisable to pour water over the three "earths" used. Measure the resistance between each set of "earths," and in this way obtain three independent equations containing the three resistances of the three "earths," and the known resistances of the three leading wires going respectively from each earth to the testing arrangement. From these three equations the required resistance can be found, and the question would be completely solved did the earth circuits behave as simple metallic circuits. This is, however, not the case, for in the first place an "earth" long used for telegraphic purposes frequently acquires a highly polarised state giving rise to a current. Secondly, if the "earths" employed are not of the same material, for instance, one an iron post, and the other a copper plate, they will form a galvanic element with the ground giving rise to a current, and lastly the testing current itself polarises the "earths." Consequently the measurement of the same set of "earths" taken successively with positive and negative currents will not agree, and they will differ from each other much if the current due to the "earths" is considerable in

comparison with the testing current itself. To obtain the real means in each case it is, therefore, necessary to employ the same sort of equations as were used in the case of line testing; the formula, however, in this case being somewhat more complicated, since the resistance of the testing battery cannot now be neglected in comparison with the other resistances as is usually done in ordinary line testing.

The "earths" in many cases having been shewn by testing to have a somewhat high resistance, they are now all made as follows:—A copper plate, four or five feet square, is buried at a convenient depth, and the connection with it made by a wire insulated from the ground. The object of insulating the wire is to prevent it becoming highly polarised, as it would were the greater portion of the current to escape from its comparatively small surface, since, of course, the chemical action produced at any surface is (other things being the same) proportional to the whole current escaping divided by the area of the surface.

Signalling Instruments.

The receiving instrument employed in India is what is called a sounder, that is, a simple electro-magnet, of which the armature is held back by a spring. For each current passing through the electro-magnet two distinct sounds are produced by the armature striking against two stops, which limit respectively its downward and upward motion. If the signalling current be of short duration, these two sounds follow one another in rapid succession, and the signal produced is equivalent to a dot. When the signalling current is of longer duration, the interval separating the two sounds is longer, and the signal is equivalent to a dash. The advantage of the sounder over the ordinary Morse recorder, or any other instrument read by sight, is that it is so very much more easy for the signaller to hear the instrument and write down the message, than to have perpetually to look backwards and forwards from the receiving instrument to the paper. It is just as much easier for a signaller to receive with a sounder than with a Morse recorder, as writing from dictation is easier than writing from a copy. The only case where Morse recording instruments are used in India is where messages are being received from, or are *en route* to places outside of India, Burmah, or Ceylon. To facilitate this reading by sound, the receiving signaller gives an acknowledgement by sending a dot at the end of every word, and the sending

signaller continues repeating the word until he gets this acknowledgement. This is the same plan as is adopted with the ordinary needle instrument. This, of course, prevents the necessity of asking for repetition of various words at the end of the message. There might be a little difficulty in using this plan with the Morse instruments in England, as on account of the double current system that is extensively used in this country the line is disconnected from the key by a switch while receiving. This switch is dispensed with in India, as only the copper current is used. On all circuits, long and short, Siemen's polarised relays are employed, and this is obviously the most efficient and economical mode of working.

It is possible, of course, to produce good signals in one or other of two ways, either by using a strong signalling current, and then the relay may be dispensed with, or by using a weak current, and working the receiving instrument with a strong local current generated in the receiving office. Now on every line a certain percentage of the current leaving the sending station is lost, therefore the smaller the current leaving the sending station the less is the absolute loss, and not only this, but the percentage of current lost on any particular line increases somewhat with the current, Ohm's law not being strictly true for leakage of insulators, or, perhaps, rather the resistance to surface conduction varying with the current. Consequently, there is a double gain in using a small signalling current and working the receiving instrument with a strong local current generated in the receiving office.

The next point to consider, a point which I think has been much disregarded on long lines in other countries, is what should be the best resistance for the relay, to be used in each particular line. If a line were perfectly insulated, the best resistance of the relay would, of course, be equal to the sum of the resistances of the line wire and signalling battery at the other end, or, as the resistance of the battery is not large compared with the resistance of the line, we may say that if the line were perfectly insulated the best resistance of the relay should be equal to the resistance of the line. Mr. Schwendler, however, by taking into account the average per mileage leakage on the different lines in India, has calculated that the resistance of the relay should be about $\frac{5}{8}$ of the true wire resistance of the line, and consequently the relays for the different lines are now made in accordance with this rule. We have not, however, yet used any relays having more than about 3,600 or 4,000 ohms resistance.

Of course, in using this rule of Mr. Schwendler's, the farthest distance from which the relay will have to be worked without translation must be considered, as that will be the case in which the current will be weakest, and when it will be most important to have the best resistance for the relay.

As the plan of receiving an acknowledgment after every word sent, is, as I have already explained, used in India, the line cannot be disconnected from the receiving instrument during sending. The static charge which accumulates in a line when a current is sent, and which is always discharged at both ends of the line will, therefore, be discharged partly through the relay of the sending instrument. If the line be very long, and the static charge, therefore, considerable, this return current, as it is called, may be strong enough to work the relay, and produce a signal at the sending station. Every signal that the signaller sends is, therefore, repeated on his own instrument, and this is very likely to prevent him recognizing the acknowledgment sent by the distant station after each word, as he would confuse it with the return current. To avoid this a key was for some time used, which put the line to earth between the two positions in which it put the line to the battery and to the relay respectively. To do this effectively required that the back portion of the lever of the key should move through a considerable space, if it was to be moved easily with no greater manual force than is ordinarily used for signalling, or else that a considerable force should be employed if the handle were only to move through the ordinary space. Either of these conditions was, of course, incompatible with rapid signalling. This scheme was, therefore, abandoned, and the following more ingenious discharging arrangement adopted. The line is connected in the ordinary way with the axis of the key, the copper pole of the battery with the front stud, and one terminal of the relay with the back stud, the other terminal of the relay and the zinc pole of the battery being connected with the earth. Between the copper pole of the battery and the front stud of the key there is inserted a Siemen's polarised relay, called a discharging relay, the tongue of which is always connected with the back stud of the key. When the signalling current is sent, the tongue of this relay is moved slightly by attraction and put in connection with the earth, in consequence of which the back stud of the key is also connected with the earth and the relay belonging to the receiving instrument is short-circuited. Now, on

account of the residual magnetism in the relay of the discharging arrangement, this state of things is maintained for a short time even after the signalling current has been broken. When, therefore, the lever of the key which is attached to the line is disconnected from the front stud, and allowed to come in connection with the back stud, it finds the relay of the receiving relay short-circuited, and which remains short-circuited for a sufficient length of time for the line to discharge itself; no signal, therefore, is produced at the sending station by the return current. Before, however, the acknowledgment arrives from the distant station, the residual magnetism of the discharging relay, which lasts but, of course, for a short time, has ceased, the back stud of the key is disconnected from the earth, and the relay of the receiving instrument is fit for receiving. To increase the residual magnetism of the cores of the discharging relay, its coils are shunted. This, of course, diminishes the sensibility of this relay, but enables the momentarily induced current, generated on breaking the battery circuit in signalling, to be utilized in prolonging the effect of the signalling current on the discharging relay. The best resistance of this shunt, as Mr. Schwendler has lately determined from mathematical considerations, is equal to the resistance of the coils of the discharging relay.

When a portable receiving instrument is required, a very compact arrangement in the form of Siemen's polarised relay is employed, the tongue of which, in this particular case, is made of sufficient bulk to produce audible sounds in striking against the stops that limit its motion. A small signalling key is attached to the coils, the whole forming an instrument that can be put in the pocket, but still which can be used in signalling as far as two or three hundred miles, and which is immensely superior to any form of galvanometer, in that it can be used in the dark. I remember on a particular evening when I had no lantern with me having a lead attached to the line wire and communicating with Bombay, from which place I was many miles distant. This I should have been prevented from doing on account of the darkness, had I had to use any form of detector galvanometer.

In order to detect the presence of signalling currents that are either too weak or too strong to work the polarised relay, the general plan adopted is to insert a small galvanoscope between the line and the receiving instrument. To avoid, however, the introduction of this unnecessary resistance, the method employed in India consists in

placing on the top of the relay a small light horizontal magnet balanced on a pivot, and which is deflected by very slight alterations in the magnetism of the cores of the polarised relay.

Batteries.

The cell in universal use in India is the Menotti or modified Daniel. It consists of a round earthenware glazed jar about five inches high, at the bottom of which is placed a disc of copper, to which is attached an insulated copper wire. Above the disc are put about eight ounces of crystals of sulphate of copper, above this sand or sawdust, and lastly at the top a zinc disc. The sand or sawdust is useful in preventing the sulphate of copper from rising to the zinc plate. The electromotive force of this cell is remarkably constant, and the resistance, when the cell is in good order, varies from 10 to 20 ohms, depending on the amount of sand or sawdust, and the tightness with which it is packed. The resistance is, of course, of little consequence when the cell is used, as it is in India, on lines, the wire resistance of which is from 1,000 to 7,000 units, or more. For local circuits these cells are also used, but joined partly in parallel circuit, that is copper to copper, &c., and partly in series, that is copper to zinc, &c.

The sounders are now all made of about 30 ohms resistance, but they have been made at different times of all resistances from 3 to 30 units. In each office the sounders are divided into sets according to their resistances. Each set of sounders is worked with a distinct local battery arranged according to the number and resistance of the instruments forming the set, the actual arrangement in each case being determined by printed rules given to each office. These rules are calculated from the fact that a single sounder of the ordinary size, wound with wire so as to have about 30 ohms resistance gives good signals with four Menotti cells in series. Such sounders, it is clear, could each be worked with a portion of the line battery belonging to the particular instrument, so that in an office having no sounders of less than about 30 ohms resistance no local batteries need be necessary.

All batteries are tested three or four times a week for electro-motive force and internal resistance, and the results recorded in a book to be examined by the Superintendents and Assistant Superintendents on their periodic visits. All new cells are also similarly tested before

being joined up in any battery. The instrument used for this battery testing is a peculiar kind of tangent galvanometer, designed by Mr. Schwendler. This galvanometer is wound with a thick and a thin coil, and has attached to it two other coils of wire of suitable resistances, which can at pleasure be put in circuit or not with the two galvanometer coils respectively. By this instrument the internal resistance of a battery can at once be found in Siemens' or B. A. units, and the electro-motive force in terms of a standard cell. It possesses also the following advantages:—

(1) Considerable sensibility, owing to the magnet being light and well balanced, (2) that it is compact and very portable, (3) that it can be used as a receiving instrument for strong or weak currents. A detailed account of this instrument may be found in a paper that I read before the Asiatic Society of Bengal, and reprinted in the Engineer.

The number of cells to be used in the line batteries of the different circuits is obtainable from the following rule:—

			During Dry Season.	During Wet Season.
For every hundred miles of		No. 1 wire	4	6
"	"	No. 3 "	5	8
"	"	No. 4 "	6	9
"	"	No. 5½ "	8	12
"	"	No. 8 "	12	18
"	"	No. 9½ "	16	24
"	"	No. 12½ "	32	48

which with the average insulation of the lines in India has been found from experience to allow a considerable margin above the minimum number of cells necessary to produce good signals. Of course, in using this rule, the farthest distance the battery has to work direct must be considered. In addition to this, information is also given to each office of the lengths and gauges of the various sections of all the lines in the division of India in which that office is situated; so that every telegraph master is in a position to decide what number of cells he should use in the dry and in the wet weather, respectively, for each line.

When a very portable battery is required, as for instance, when a man proceeds out on interruption duty, the following arrangement, originally devised by Captain Mallock, is employed: an oblong wooden

box about a foot long, three inches wide, and three inches deep, is subdivided into cells by divisions which themselves consist each of a zinc and copper plate soldered together. The cells are filled with sand moistened with dilute sal-ammoniac. This battery is of course not particularly constant, but is handy and portable.

Every office is supplied with a Swiss commutator, consisting, as most present know, of a series of vertical and horizontal metal bars, all insulated from one another, any one of the vertical bars being able to be connected with any one of the horizontal bars with a screw plug. By this arrangement any line can at once be connected with any instrument, or direct to any other line, or to earth, or to the Wheatstone bridge, or differential galvanometer, if the office be a testing station. The metal bars should be fixed on an ebonite, and not a wood backing, as is sometimes done, as the wood warps and then the holes in the vertical bars do not coincide with those in the horizontal. At every office also there is a Siemen's plate lightning discharger of the proper size for the maximum number of lines to come into the office. By a late universal order the lightning discharger at every office is placed between the line and the commutator, and not as formerly between the commutator and the instrument. This ensures two important results: 1st, that the commutator is preserved from atmospheric discharge; 2nd, any leakage in the lightning discharger, caused by dirt or otherwise, will be discovered from the ordinary insulation tests of the lines which are made from commutator to commutator.

DISCUSSION ON THE PAPER.

The CHAIRMAN having invited discussion,

Mr. W. H. PREECE asked what was the normal state of insulation in India in dry weather and in wet weather.

Mr. AYRTON replied in dry weather it varied from 5 millions a mile to over 300 millions; in wet weather it was as low as half a million a mile. He should say the average was 2 to 3 millions in wet weather, and, perhaps, 40 millions in dry.

Mr. PREECE asked what instrument was used in India for making the daily and periodical tests.

Mr. AYRTON replied the Wheatstone bridge was usually used: in some cases the differential galvanometer. The latter was being superseded.

Mr. PREECE asked what galvanometer was used.

Mr. AYRTON: The static reflecting galvanometer.

Mr. PREECE: Are the tests made daily or periodically?

Mr. AYRTON: In the large offices daily, in those not large perhaps three times a week.

Mr. PREECE: At any particular hour?

Mr. AYRTON: They are usually made early in the morning—6 or 7 o'clock—generally before 9 o'clock, except on Sundays, and then during every portion of the day, as messages are not taken.

Mr. PREECE: What is the length of time of testing?

Mr. AYRTON: The actual testing perhaps 10 to 15 minutes. Of course the calculations take much longer.

Mr. PREECE said he had asked these questions, because, in practice, the system adopted in India for testing lines of telegraphs did not differ materially from that which was adopted in England, but the conditions in the two countries were totally different. Here we were compelled to confine our tests to certain hours and to make them as rapidly as possible. The rule was to test all important lines at 7.30, or 8 a.m., and it was confined to 10 minutes or a quarter of an hour. The result was, that in large offices with 30 or 40 wires, they had to test them all in 15 minutes; and, of course, where they were so confined to time they were compelled to adopt measures far less accurate than those described by Mr. Ayrton. The system of testing lines daily was simply a rough test of their condition. If by that means a wire was found faulty, a more accurate test must be made to localise the fault. In order to obtain those accurate data which were essential to the electrician to know the condition of his line, periodical tests were taken, sometimes once a week, sometimes once a fortnight, and in dry weather once a month. They then went through all the details mentioned by Mr. Ayrton, and they endeavoured, to the best of their ability and knowledge, to eliminate the evils arising from earth currents and other matters. In England the effects of earth currents were not so severe as in India. It was sufficient here to take the mean of positive and negative observation, or where taken with the differential galvanometer, making the deflection of the earth current zero. That gave within 1 or 2 per cent., the necessary accurate observation.

The great point, in the system of daily tests, which they endeavoured to arrive at was, to obtain the earliest information of the

condition of the lines, so that wires which failed during the night could be put into order before business commenced. If a wire be broken down between the hours of 10 a.m. and 12 noon, when that wire ought to be full of messages, of course, it acted in a very detrimental manner to the business, so that every effort was made to remove the fault before the heavy hours of business commenced, and the principal object of making the test was not so much to give a knowledge of the electrical condition of the line, as to localise faults, and to be able to give instructions to the men to proceed to their repair. There was one other question with reference to earth plates. Mr. Ayrton had told them very accurate methods were used to test earth plates, to find out what resistance they gave; but he omitted the essential information, viz: the nature of the earth plates experience showed was necessary in India. In England they used the gas and water pipes where it was possible to do so: but what did they use in India?

Mr. Ayrton: A copper plate 4 or 5 feet square with wire insulated in the ground.

Mr. Preece: What resistance do you find that gives?

Mr. Ayrton: It varies considerably with the soil from less than one unit to 30 units. One plate buried vertically 10 or 12 feet deep, was taken up and buried horizontally about a foot below the surface, and the resistance was reduced to 5 or 6 as compared with 30 at the greater depth below the surface. He should say on the whole the resistance was 2 or 3 units when kept moist. To keep the plate moist an ordinary telegraph pole was sunk beside the earth plate and water was poured down a tube close to the earth plate.

Mr. Preece was pleased to find the system of sounders was being introduced into India, inasmuch as on one or two occasions he had advocated before the Society the use of sound reading. That system had been introduced almost entirely in America, and was becoming largely used in India. We are also using it to some extent in England, and he was quite sure that it was the instrument of the future, and that before long we should benefit by the experience of India and America and adopt it to a large extent in this country.

There was one portion of Mr. Ayrton's paper which he heard with great pleasure, that was, that in India they had introduced the method of working with double currents. For a long time England was unique in working with double currents. We had done it since 1854,

and it had been greatly extended of late years, and he was satisfied when other countries found the benefit of it they would adopt it likewise. There was only one objection he knew of to the use of double currents, and that was the necessity of using a switch to turn the speaking instrument on to the line and off. He had never found any objection to that in practice, and the objection existed more in imagination than in fact. There was, indeed, no objection to its use while there were on the other hand great advantages in the double current system. One great advantage was, that however long the circuit which they worked with one current was, they could work a circuit twice as long with double currents, for this reason, that in the double current working we eliminate the antagonistic forces in the relay and the working current had simply to move the tongue of the relay and nothing else. Another advantage was, that in instruments worked with single currents they had constant antagonistic forces working against variable moving forces; with the double system both forces varied equally, the practical result being that long lines in England, which are easily worked with double currents, could not be worked advantageously or even at all with single currents. He was sure when the double current system was tried more the advantages would be found so great that for all long lines it would be generally adopted. We had in England adopted a method of apportioning given resistances to our relays, and the practice was to make the resistance of the relays bear a definite ratio to the general average length of the circuits. We could not make the instruments agree with every circuit, because the circuits were so numerous and varied so much. He considered the Society was very much indebted to Mr. Ayrton for his excellent paper, and he was sure the members would peruse it with great satisfaction when they received it in the *Journal* of their proceedings.

Mr. AYRTON in reply to the observations of Mr. Preece, said that gentleman had stated that the system of testing in this country was almost the same as that adopted in India, except in so far as it differed on account of the different conditions. One condition was not very different on the long lines of India and the long lines in England. He did not know what was done in testing now; but two or three years ago no attempt was made to separate the real resistance of the wire from the real resistance of the insulators. Mr. Varley mentioned to him a line which he tested in which the line insulators

at the end had not so much apparent resistance as the wires had; so that the result he obtained from the insulators did not tell what was the resistance of the wires and the insulators respectively. The result of this was, no attempt was made to separate the wire resistance from the insulator resistance, because it was known that a line was not uniformly insulated, and also that there was a good deal of leakage from the wires in tunnels. As he had said, where a line was not equally erected it was difficult to separate line resistance from wire resistance. There was no easy method of doing it but by calculating the resistance of the relay, and then seeing whether it was greater than, or equal to, the real resistance of the relay. When a line was uniformly insulated—and by that he did not mean every mile the same insulation—the leakage on each side of the centre was the same; and when that was the case, the resultant fault would be near the centre. The equation he gave showed that. The simple equation would give accurate results, differing very much from apparent tests. He did not know whether the test of the line, including the relay at the other end, was generally taken. It was sufficient, perhaps, to find where the resultant fault was, and whether the equations gave a value too high or too low. Equations were not used to separate the wire resistance from the insulator resistance. Mr. Preece mentioned that the object of testing the lines early in the morning was not so much to give an electrician a knowledge of the normal qualities of the line, as to test the faults. If a fault in India was sufficiently bad to interfere with work, it was tested for at once, whether during day or night. Every fault was tested for, if the signaller said his signals were so weak that he could not receive them, within an hour of the discovery being made. A person qualified to test resided near the office if it was a testing office. The tests of the morning were made, not to find out faults, but to ascertain the normal state of the line, which was a most important consideration, because certain lines were usually better insulated than others, and the good portions were joined up together for long services, and the bad were used for local work.

With reference to the double current system, there was the disadvantage of using a negative current; for if the line had points of leakage the current was diminished. If the leakage was distributed over the line the current would not be materially diminished by the negative current, which was due to chemical action, which cleaned

the wire by putting *hydrogen at the leakage point.* Though the whole current escaping was low at one point the chemical action at that point was inconsiderable, but if it escaped at one or two points the chemical action was considerable, and the leakage would be considerable; but where the leakage was at only one or two points they did not use a negative current. With a cable there was reason to use negative and not positive current, because positive current tends to eat through the copper wire, and could be worked with a bad insulation so long as the copper was entire. The only thing was interruption in the wire. In a cable it was better to use negative current to signal with than positive, if there was a fault, because the positive, though it increased the insulation and did good, would eat through the copper wire, and in time destroy insulation. But in land lines the great point was insulation; therefore it was better to have good insulation, and not to use negative current, if there was no occasion, on any land line on which the leakage was at a few points. Another disadvantage of positive and negative current was it necessitated the use of a switch, and if that was used the acknowledgment would not be given at the end of each word. It greatly interfered with work to have to use the switch, and he thought they ought not to abolish the plan of giving an acknowledgment after every word: otherwise there was no power to stop the sender, and a great part of a message might be sent without being received. The next point was, what was the object of using double currents? Mr. Preece said they could work twice as far with double current as with single. They could work almost as far as they wanted with single current. They could work 1,000 miles with single if the line was in respectable order; therefore there was scarcely any necessity to use double currents. Then again, Mr. Preece said the relays were made according to general average. That was not the point he (Mr. Ayrton) drew attention to, but that they were made for the particular line. Of course, the lines did not vary so much here as in India. They had lines of 10 miles and lines of 1,000 miles, so that a relay for 10 miles would not do for that for 1,000 miles. The relays varied from 200 units to nearly 4,000 units—200 units for lines from 20 to 80 miles long, and 4,000 units for circuits of 900 or a 1,000 miles; and even that relay was not according to the rule he gave, viz., of $\frac{5}{8}$ths of the resistance of the line, &c. Therefore the resistance of the line should be 10,000 units, which was much greater than 4,000; but to make relays of $\frac{5}{8}$ths would be

expensive. They had two sizes of relays in India: a small size being used for the short lines and a much larger size for the longer lines.

The CHAIRMAN, in closing the discussion, said it was impossible to have listened to this paper without contrasting the state of the Telegraph in India at the present day with what it was 10 or 20 years ago. He remembered when it was first taken up by Sir Wm. O'Shaughnessey. He believed he was quite alone in creating it. He had heard of telegraphs in England, but had no means of knowing what was done, and he was left entirely to his own knowledge. He believed at that time the Telegraph in India was the roughest thing possible. Since then the system had gradually grown up, knowledge had increased, and more recently Colonel Robinson came to this country to study our system; he saw the great advantages which were derived from the use of scientific methods in this country, and resolved to introduce them into India, and with what results they had heard this evening. He believed at the present time, the Indian Telegraphs were amongst the most scientifically worked telegraphs in the world, and it had been to him very interesting to hear a description of the system adopted in that country. There were many questions of practical interest he would have liked to have asked, but at that late hour he would not do so. He thought much credit was due to Col. Robinson, for the extraordinary improvements he had introduced into the Indian Telegraphs, and that they deserved to be publicly and very warmly acknowledged.

The vote of thanks to Mr. Ayrton having been passed unanimously the meeting adjourned.

The following Candidates were balloted for and declared duly elected:—

AS FOREIGN MEMBER:—

Mr. C. L. Madsen, Great Northern Telegraph Company, Copenhagen.

AS MEMBERS:—

Mr. W. H. Ashurst, General Post Office.
Mr. Edwin Clark, M.I.C.E., 5, Westminster Chambers.
Major Alexander Moncrieff, Athenæum Club.
Mr. George Saward, 66, Old Broad Street.
Lieut.-Col. Henry Schaw, R.E., Staff College, Farnboro'.

As Associates:—

Mr. E. Bayley, Postal Telegraph, Hull.
Mr. Henry Cox, 48, Arthur Road, Holloway.
Lieut. Sale, R.E., Chatham.

As Student:—

Mr. Alfred Hayes, 2, Westminster Chambers.

The Sixteenth Ordinary General Meeting was held on Wednesday, April 9th, 1873, Lieut.-Colonel R. H. Stotherd, R.E., Member of Council, in the Chair.

Mr. W. F. King read a Paper "On a Bell Alarm for Submarine Cables."

In order to save time, it is the practice on some submarine lines, to send several messages in succession without waiting for an acknowledgment. But this system frequently causes delay in another way, for, with the ordinary connexions, the receiving clerk cannot interrupt the sender while signalling, and, accordingly, if a break-down occurs, he must wait till he has finished, and then everything has to be repeated.

The object of the instrument I am about to describe is to obviate this evil, by giving power to the receiver to interrupt the operator at the other end by means of an alarm bell, which he can ring at any moment.

To make what follows clear, it will be well to describe briefly the usual connexions for speaking through a cable. They are shown in the Diagram I. B is the sending battery (of from 5 to 20 Daniell's cells) joined to the key A. C is a condenser, whose capacity is generally between a fiftieth and a tenth of the capacity of the whole line. One of its sides is connected direct to the cable D, and the other through the switch S, either to the sending key A or the receiving instrument G. The connexions are the same at the other end of the line. It will be perceived that the cable is always insulated.

The instrument which forms the subject of this paper has been in use for some time on the St. Pierre-Duxbury section of the French Atlantic Cable, and works satisfactorily. It is a relay consisting of

DIAGRAM 1.

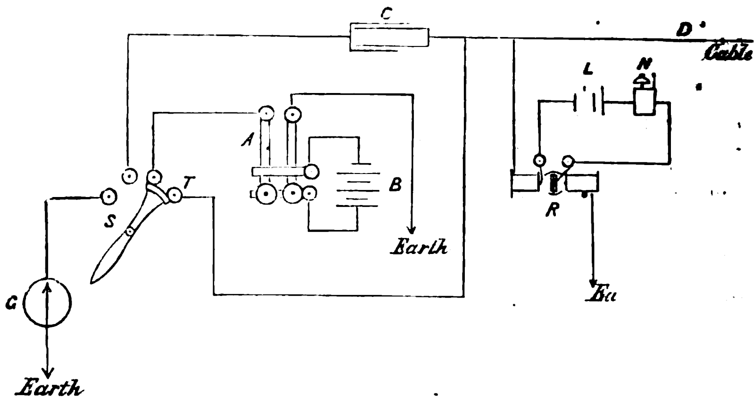
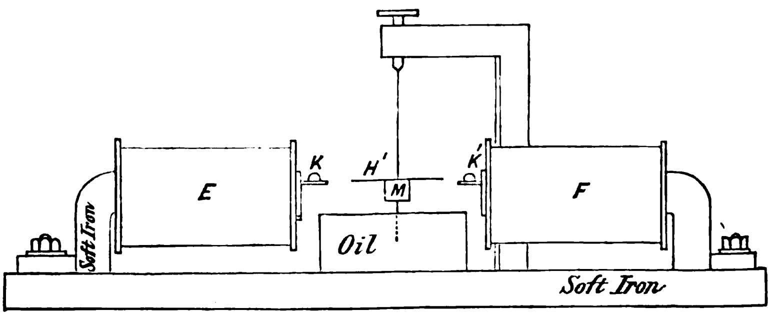
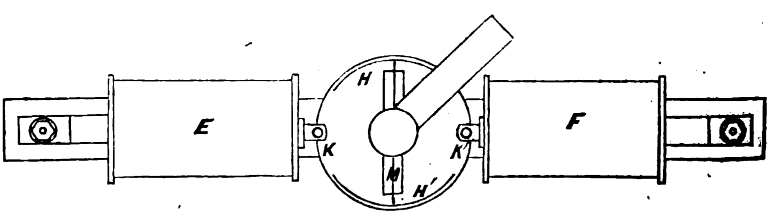


DIAGRAM 2.

Elevation.



Plan.



BELL ALARM.

a horse shoe electro-magnet, with its poles bent round to face each other as shown in Diagram II. The bobbins E and F are wound with thin German silver wire, and the sum of their resistances should be about a hundred thousand ohms. A steel magnet M is suspended by a fine platinum wire P, between the poles of the electro-magnet. The magnet would have a tendency to set along the axis of the bobbins, but the wire P is made sufficiently stiff to keep it in a position at right angles to that, and to bring it back from a considerable deflection on either side. The moment of inertia of the needle, M must be so great that its period of vibration shall be at least ten seconds. Just over the magnet, and rigidly attached to it are two curved platinum wires, H and H′. When the magnet is deflected about 20°, each wire makes contact with the two globules of mercury K and K′. The contacts are thus made double, in case one of them should fail, for a perfectly reliable contact is essential. The globules of mercury K and K′ are connected to one pole of a local battery L, the other pole of which is joined to an electro-magnetic alarm bell N, which again is in connexion (through the suspending wire P) with H and H′. Thus when either H or H′ makes contact with either K or K′, the local circuit is completed and the bell rings. The magnet M, or a vane attached to it is immersed in a vessel of oil O (diagram II) so that the whole system is heavily dragged. This is done to prevent the currents sent through the relay, in the ordinary working of the line, from gradually getting it into a state of vibration.

The extra connexions required when this instrument is used, are shown in Diagram I. The relay R is permanently inserted between the cable D and the earth. Because of the great resistance of the relay this does not affect the speaking through the cable. On the other hand speaking through the cable does not affect the relay, because the short currents and quick reversals acting on its slowly vibrating magnet counteract each other and keep it nearly at zero. Even were the sending key to be held down for a long time, as the condenser is never greater than one-tenth of the capacity of the cable, the cable could only be charged to one tenth the potential of the battery, and this is not sufficient to deflect the relay enough to make contact. The intensity of the currents received being seldom more than a hundredth of the intensity of the currents sent, their effect on the relay is of course inappreciable.

By means of the terminal T in the switch S, Diagram I, the sending

key and battery can be connected direct to the cable. By depressing one of the springs of key A, the whole line is charged to the full potential of the battery, because the resistance of the two relays—one at each end of the line—is practically infinite, compared with that of the battery, and the cable otherwise is insulated. Thus the full difference of potentials of the battery is maintained between the ends of the relays, which are so adjusted that this electro-motive force is only sufficient to deflect them far enough to make contact and ring their respective bells. The clerk, wishing to interrupt, must charge the cable first with one pole of the battery, and then with the other; for, if there is an earth current opposing him while he used the first current, it will assist him when he uses the other. Thus, earth currents can never prevent the bell being rung.

This brings me to an objection which has no doubt suggested itself to many of those present, viz., that the earth currents will be sometimes strong enough to ring the bell. This does occur, but so unfrequently as not to occasion much inconvenience.

On all submarine lines, but more especially on any in which a fault exists, it is advisable that those in charge should have warning when the potential rises to such a pitch as to endanger the cable. This I propose to accomplish with the relay described. In times of electrical storms, which would be well marked by the action of the relay in its ordinary condition; I propose to diminish its sensibility by reversing the current through a portion of the coils composing it. In this way the bell would only be caused to ring when the potential of the line reached a certain definite height—say 100 or 150 cells. On this potential being attained, such steps as are considered most advisable would be taken to prevent injury to the cable.

An instrument to give warning in case of excessive earth currents has, I believe, been proposed, if not also used, by Mr. C. F. Varley.

The third use to which this relay may be applied is a very obvious one, viz., to rouse attention at any station when a communication has to be sent after a cessation of business.

The following Paper was then read "On the Mechanical Testing of Telegraph Wires," by R. S. Culley, Member of Council, S.T.E.

Wire for telegraphic purposes should possess considerable ductility so as to bear bending well, and to stretch under an accidental heavy strain.

Two methods of testing for ductility are largely employed, and by far the greater part of the wire made for telegraphs in England which is tested at all, is tested in one or the other mode. The first method, that formerly employed by the Electric Companies and now by the Post Office, is by stretching; the second, adopted by the Indian Telegraph Department, is by twisting; in the first case, the wire must bear a certain percentage of elongation (18 per cent.), and in the second case, a certain number of twists in six inches before breaking.

Manufacturers generally prefer that the elongation test shall be made on a length of 10 inches; I shall be able to show that while in the hands of practised experimenters this test is a good one, and this is fully proved by the excellence of the wire selected by it, a 10-inch length is not sufficient for general purposes.

The percentage of elongation is affected—

A. By the *length* of the piece tested.

B. By the *time* occupied by the test.

It is impossible to manufacture a coil of perfectly uniform quality throughout, and therefore, the greater the length of the piece tested, the greater will be the chance of its containing a section of low ductility.

For example:—

Seventy-five tests of a coil of ·165″ diameter wire gave the following means:—

10 in. pieces stretched	...	19·5 per cent.
120 in. " "	...	12·7 "

But a few tests of 100 yard pieces gave a very much lower result, the elongation being only little over 6 per cent. before breaking.

Taking two qualities of wire ·239″ diameter, one much better liked by telegraph workmen than the other, both were equally well able to pass the 10 inch test, elongating 18 per cent.

But in 10 feet pieces—

A stretched	...	12·0 per cent.
B "	...	9·0 "

And in 100 yard pieces—

A stretched	...	5·5 per cent.
B "	...	4·5 "

The longer pieces here brought clearly out the relative qualities of the iron, which tested in short pieces seemed almost identical.

The 10 feet length gives in practice trustworthy results; 100 inches, or possibly even 50 inches, might be sufficient to bring out those qualities which exist in a wire easy and pleasant to handle in erection, and free from the danger of breaking when erected.

In both the 10 feet and 100 yard pieces the wire remains ductile except immediately on either side the break; in one case, a wire, which tested in 100 yards broke at 5 per cent. elongation, bore 8 per cent. more when afterwards tested in 10 inch pieces.

B. Time.

The amount of elongation a wire will bear before breaking is, roughly, inversely as the time occupied by the test. Thus—

When *times* were as	1·00	...	1.73	and	3·11
The *elongation* was	17·80	...	15.40	"	13·80

Mr. Bell, who has had great experience in wire testing, thinks that the heat developed by rapid stretching softens the wire, and enables it to stretch more freely.

The same result as to the effect of rapid testing is observed in breaking wire by scale and weights, the wire stretching more if the weights are put on quickly than if it is allowed to complete the elongation due to each successive increase of weight before the weight is further increased.

2. The Twist Test.

The Indian Telegraph Department tests for ductility by gripping six inches of the wire between two vices and twisting it. The number of twists is shown by the spiral, formed by a line drawn with ink along the wire previous to the test.

This test is affected by the time occupied by the twisting, but not

to a great extent. The means of 100 trials in each of two machines gave the following results—

Speed = 10 and 25.
Number of Twists = 14 and 14·3.

The heat developed in this test is slight as compared with the former; for in this only the outer fibres are violently displaced.

The best qualities of charcoal and of homogeneous iron bear the twist better than the elongation test. Thus comparing wires of the size—

Ordinary wire,	Elongation	17·4	per cent.,	No. of Twists in 6 inches,		12.
Charcoal	„	17·0	„	„	„	18.

Again, with wire nearly alike in size—

Ordinary wire	·234 in.	Elongation	17·6	per cent.,	No. of Twists,	10.
Homogeneous	·253	„	17·6	„	„	13.

The diameter of the wire affects the torsion test because the angular displacement of the outer fibres increases as the diameter. The number of twists a sample will bear is roughly inversely as the diameter.

The average of a large number of trials gave 13 turns in 6 inches for a wire ·253 inches diameter. Taking this as a datum, the number was calculated for other smaller sizes and compared with experimental results—

Diameter	·253,	actual number of	turns in 6 inch,	13.0,	calculated	13.0
„	·207,	„	„	15·2,	„	15.9
„	·146,	„	„	24·9,	„	22.5
„	·077,	„	„	38·7,	„	42.7

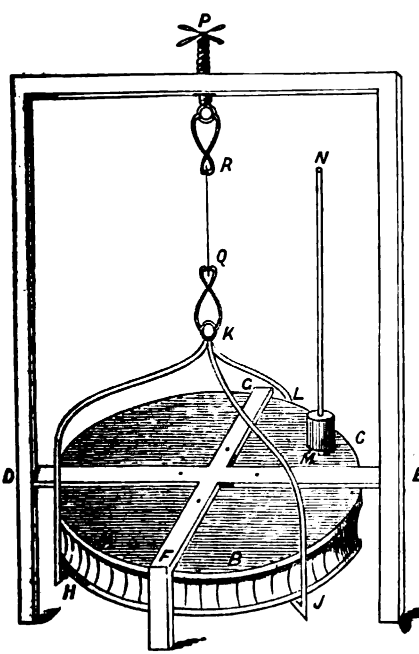
A series of trials have been made of elongation and torsion on the same sample of wire, but as the elongation was taken on 10 inch pieces the results were variable. It is desirable that the experiments be repeated with pieces of 50 inches, or whatever other length be decided on to replace the present 10 inch piece. The results will then it is to be hoped settle the point in question, whether torsion be as effectual as elongation in securing the quality required, bearing in mind that a test should be, as far as possible, such as can be applied without special skill, practice or experience.

In preparing this Paper I have been most materially assisted by the remarks and tabulated results of tests made by Captain Moxon, the Inspector of Wire for the Indian Telegraph and General Post

Office, and by Mr. ANDREW BELL, formerly of the Madras Railway Telegraph, and now the Storekeeper of the Post Office Telegraphs, who at one time served under Captain MOXON.

Mr. W. E. AYRTON said:—In dealing with the tests for ductility of wire, Mr. Culley has only referred to two, the one by measuring the percentage of elongation before breakage, the other by observing how many times a fixed length can be twisted round its own axis before it snaps. The latter of these, "the torsion test," as it may be called, Mr. Culley has shewn gives more trustworthy results than the former. There is another test, however, which is a better criterion of the ductile power of wire, namely, that of bending it round a fixed diameter and unbending it again, the unbending being the really important part of the test, as the reverse action shews flaws that are not discovered by merely elongating or bending. I have not unfrequently seen specimens of wire that would not stand this latter test, but of which the percentage of elongation was even greater than the average percentage. It, therefore, appears that elongation tests really only measure the stretching power of wire, and not its power to bend. This bending and unbending test has, however, one decided disadvantage, that it only tests a very small piece of wire. In the case, however, where the bending and unbending test has been most extensively employed, namely, in testing the homogeneous wire for the cables of the "Great Western Telegraph Company," it was found that there was frequently a variation in the quality of the wire in the same hank, so that neither the test of a few inches nor of some yards could determine absolutely the quality of the wire throughout the whole hank. Where, however, any doubt existed regarding the ductility of any particular hank, this bending and unbending test was applied, not only to one piece, but to a number of pieces cut from each end of the hank. In addition, the tensile strength and the percentage of elongation of the end of every hank of homogeneous wire that was used were measured and recorded. The tensile strength and elongation were simultaneously measured on a piece ten inches long. It was fully understood that it might have been better if a longer length had been employed, but the ten inches was adopted on account of the strong opposition of the wire-drawers to any other length. For straining the wire, a hydraulic machine, constructed by Sir Wm. Thomson, was employed. The machines, commonly used for wire testing, are either simply dead weights, in which case it is

exceedingly difficult to apply the force gradually; or hydraulic pumps, which have the same objection as in the former casé, and in addition are liable to get out of order through faults in the valves; or lever machines, in which errors may arise from friction; or spring machines, which frequently give inaccurate results from the spring being out of order. Now, in the machine of Sir Wm. Thomson, to which I have referred, the force can be applied as gradually as is desired, and there are neither valves nor springs, nor is there práctically the slightest friction. A short description, therefore, o this apparatus may perhaps be interesting to those who are present. (A B C) is a disc of wood about an inch thick and three feet diameter, rigidly attached to the two horizontal supports (D E) and (F G). (H I) is another similar disc of wood, supported by the three metal bars (K H), (K J) and (K L), the latter of which is only partially seen in the figure. This lower disc is attached to the upper one by a band of india-rubber as in a bellows. (M N) is a glass tube open at both ends, passing through a hole in the upper disc and rigidly attached to it. The space between the two discs of wood is filled with water. When the handle (P) is turned, (K) will be raised, this would tend to compress the water in the bellows, which will consequently rise in the glass tube. The height to which the water rises tells the pressure per square inch on the lower disc, this multiplied into the area of this disc gives the whole strain on the piece of wire (Q R) under test. This instrument is, of course, independent of the actual quantity of water in the bellows. When the wire under test breaks, the lower disc falls into a support placed underneath it, and so the india-rubber band joining the two discs is not strained. The object of enlarging the glass tube at its lower extremity is to necessitate the lower disc being raised an appreciable



distance before the water begins to appear in the tube, otherwise, as the diameter of the disc is very large compared with that of the tube, the water might begin to rise in the latter while still some portion of the disc were resting on the support placed underneath it, in which case, of course, the height of the water in the tube would not be a true measure of the strain on the wire under test. With this instrument tests can be made very rapidly, as the breaking strains are read off in pounds from a scale attached to the glass tube, the results are also obtained with great accuracy, as the instrument cannot possibly get out of order. Of course there is nothing new in the principle of this apparatus, as it is simply that of the old hydrostatic bellows. If, however, this wire straining apparatus were generally known, then instead of it being stated in the specification for wire for the Indian Telegraph Department, that no hydraulic machine is to be used for the testing, it would rather, I think, be made a condition that no apparatus should be employed other than the one I have been describing. The elongation is measured thus: A small scale is attached at its one extremity to the piece of wire under test, and near its other extremity there is attached to the wire a small pointer, which moves over the scale as the wire stretches. This is much better than measuring, as is frequently done, the distance between the clamps before and after testing, as this latter method frequently gives erroneous results from the slipping of the wire in the clamps.

A MEMBER asked Mr. Ayrton what was the object of the Indian telegraph authorities submitting the wire to a test to which it was not subjected in practice—that was, putting the strain entirely on the outer fibres. That was unfair to the wire, as the stretching might cause it to break there, while the interior of the structure might be perfect.

Mr. AYRTON replied he thought there could not be a better test for elasticity. It was difficult to test the precise conditions it had to sustain in practice. The bending and unbending test he mentioned as being the best, no wire had to go through in practice, and in all these tests the test was more than the wire was likely to undergo. The object of trying the torsion test was because it was considered to be the best. He thought the bending and unbending test was better than the torsion.

Mr. LOEFFLER asked whether Mr. Ayrton could give the percentage of homogeneous wire rejected upon the bending and unbending test.

Mr. Ayrton said the wire tested had a diameter of 0·091 to 0·1 of an inch. As to the percentage rejected it varied very much. Sometimes almost every bundle was passed; at other times 6 or 7 per cent. would be rejected, and he had known it to be as much as 20 per cent.; but the precise quantity rejected during the whole construction of the cable, without the records, he could not state. To the best of his recollection sometimes all the wire passed, and sometimes 6, 8, and even 20 per cent. was rejected, but not often so much as that. He was speaking of homogeneous wire. The wire was bent on its own diameter and unbent again. The specification for the Great Western cable was a little severe. It specified homogeneous wire of the comparatively small diameter of 0.95 to bear 850 lbs. It was then bent and unbent round itself. It was said it was not easy to get wire to do that, and that might account for the amount of rejection.

The Chairman having requested Mr. Muirhead to explain the method of testing in Warden's Works,

Mr. Muirhead said the wire was first tested for tensile strain between two vices, and a similar length, generally about 10 inches, was tested in the way described for twisting test—viz., the number of times the wire would twist round its own axis. The test for elongation was made upon a similar length of wire, the elongation of ordinary wire being 15 to 20 per cent.

Mr. Hooper said it was owing to the severe tensile strain they had so much of the wire put on one side. The minimum strain was 50 tons per square inch, and the average strain on the cable was 55 tons on the square inch; and under that test, combined with the bending round its own diameter, led to a great number of rejections.

Captain Mallock said it appeared from the paper two methods of testing for ductility were employed: the one by the Post Office being elongated by percentage in certain lengths from 10 inches to 100 inches; and, by the Indian Telegraphs, by twisting. He would not say by whom the latter plan was introduced, but he believed it was done by Sir W. O'Shaughnessey. The wire was at one time simply taken for the telegraph. If they wanted wire, an order was given for so many tons, which was sold as marketable wire. For some years they received wires without test; but in 1865 he had to take up the subject, and knowing little or nothing about it, he went to see what was being done by other people. He found the plan of Mr. Varley to be the running of the wire from one set of drums to another set with cog-wheels,

giving 2½ per cent. of stretch, which was killing it and practically testing it. Then, he found the plan used by Sir W. O'Shaughnessey was to twist the wire. They then introduced into their tests the combination, not of twisting and straining, but so many lbs. weight per square inch, and so many twists in a length of 6 inches. The tests laid down in 1865 of twist were taken from a number of samples from different makers, and finding the best tests the wire would give, thousands of tons of wire were bought on these specifications. After the specifications then drawn up had been working about a year, it was found that wire was being rejected which might be good wire in itself, although it did not give a certain amount of ductility in the twisting test. In taking the data obtained from that, the original specification was altered, from which it was found, taking No. 5½ wire, weighing 600 lbs. per mile, of which the diameter was 0·21, that for every 50 lbs. of breaking strain they got a twist less in the 6 inches. For instance, taking 6 inches of wire, breaking at 19 cwt., it gave a twist of 16 times, &c. Thus, from taking a number of twists made by Mr. Moxon during 7 years, he (Captain Mallock) found from practice what, perhaps, he might be told he ought to have known by theory, that the number of twists in a length of 6 inches varied inversely with the diameters. Now, about a year ago there was a proposal made that wire should be taken by every one by weight, and this taking by weight had been adopted in the Indian Telegraph Department for 7 or 8 years: that was to say, the wires they had used up to last year had all been multiples of each other. They called them by their normal engineering names for simplicity, but the actual weight they used had been hitherto 75, 150, 300, and they had added to these up to 750 and 900, all multiples of each other, and giving certain resistances in actual proportions. By getting the wires all the sizes by weight, multiples of each other, all breaking strains in twisting the wire naturally came in proportion, that was, wire weighing 1,200 lbs. per mile ought to stand double one weighing 600 lbs. per mile, and, in the twisting, the torsion being in proportion to the diameter, would be in proportion to the square roots of the weights. Starting from these numbers, from this equation, which he found borne out by a large number of tests made by Mr. Moxon, this specification was drawn up, but before that was done he went to the works, and, taking pieces of wire which he found about the place, he weighed them, and calculated what the

number of twists in 6 inches lengths would be according to weight, and he found the results thoroughly bore out this equation.

The specification to which he referred might be worth adding to the paper of this evening, but he did not suppose the meeting would wish to hear the whole of it. The greater part of the wire supplied by Mr. Johnson was furnished in accordance with this specification. Having got the one size, the proportion of twisting to breaking strain was determined, and also what he thought was of great importance; that was, that the breaking strain being proportionate to the weight, they got the same quality of iron throughout. This he had brought forward as a mere proposition which he believed, would be useful.

GOVERNMENT OF INDIA. SPECIFICATIONS OF GALVANIZED IRON TELEGRAPH WIRES FOR LINES AND CABLES.

---

1. In this specification Whitworth's decimal gauge will be used for gauging the wire, but the weight per mile is to be the only standard of size recognized in the contract.

2. The accompanying tables give the data for inspection as regards limits o size, weight, tensile strength, ductility, &c., for all sizes of telegraph wires for the Indian Government.

3. The calculated diameters of the wires are entered in the table, for aiding in the inspection, but, in the event of any dispute, the weight of 10 feet (or the $\frac{1}{528}$th part of a mile) is to be taken as giving the correct size of the wire.

4. The wire is to be highly annealed, smoothly galvanized, free from scales, inequalities, all imperfections, flaws, sand splits, and other defects, and to be cylindrical. Each piece must be warranted not to contain any weld whatever, either in the rod before being drawn, or in the wire afterwards.

5. A sample coil of the size of wire, for which a Tender may be submitted, in every respect equal to this Specification, is to be supplied by the Contractor within 14 days of the acceptance of his Tender.

6. Every coil of the supply is to be gauged for diameter, in one or more places, with Whitworth's decimal gauge, and from a number (not exceeding five per cent. of the total quantity) 10 feet pieces are to be cut and weighed. As a rule these pieces will be taken from the coils cut for the tests mentioned in para. 7.

7. Every coil of the supply may be tested for tensile strength and ductility, and five per cent. of the entire number of coils may be cut and tested in any part. Coils cut for this purpose, or for weighing samples, are not to be welded or jointed together again, but the separate pieces are first to be bound in separate coils, and then the two bound together to form a coil of the standard size (so that they may either be conveniently jointed on the work before being paid out, or that broken coils may be chosen for short lengths when required).

8. Tests for tensile strength are to be made by the direct appliance of weight vertically. No hydraulic machine or lever is to be used.

The wire is at first to lift a weight equal at least to $\frac{9}{10}$ths of the minimum tensile strength entered in the table for the size under trial, and the remaining tenth is to be added gradually, by convenient ordinary weights of not less than $\frac{1}{5}$th of the remainder, or $\frac{1}{50}$th of the minimum tensile strength. No less weight than $\frac{1}{100}$th of the minimum will be reckoned.

9. Tests for ductility will be made as follows:—The piece of wire will be gripped by two vices, and twisted. The twists to be reckoned by means of an ink spiral formed on the wire during torsion. The full number of twists must be distinctly visible between the vices, no fractions being reckoned. In sizes above 150 lbs. per mile, the vices will be six inches apart; but in those of 150 lbs. or less, they will be three inches apart, and the number of twists entered in the table have been reckoned proportionately.

10. All wire, after it has been accepted and weighed, is to be dipped into boiled linseed oil, at a temperature of not less than 100° Fahrenheit, and Tenders are to state separately the price of the "iron wire" and the charge for oiling it.

11. It is desired to obtain the wire in coils, all of one piece, of the size entered in the table. If this cannot be undertaken, the Contractor may tender for the supply of wire with two pieces only to the coil, joined either by Ryland's patent joint, or left unjointed and packed as stated in para. 7. No piece weighing less than one-third the minimum weight of a coil will be accepted. In Cable Wires these joints will not be allowed.

12. The Supervisor of Telegraph Stores and the Examiners appointed by the India Store Department may be present during the whole process of making the wire, and they shall have every facility accorded to them by the manufacturer for seeing the wire during all stages of its manufacture.

13. They shall be at liberty to see all billets or bars before rolling, and to

weigh any after rolling, in order to satisfy themselves that the separate pieces are of the full weights contracted to be delivered without joint. They shall also be at liberty to examine all wire after drawing, and before galvanizing, to see that no welds exist.

14. The testing shall be performed by the Examiners, or under their direction, and the Contractor must supply all the necessary machinery, apparatus, convenience, labour, and assistance of every kind required for the purpose without making any claim or charge for the same, it being distinctly understood that these expenses are included in the prices stated in the Tender. Waste of all kinds in testing to be at the expense of the Contractor. All testing apparatus, &c., to be immediately under the control of the Supervisor of Telegraph Stores, and the Examiners serving under his directions, during the period of examination.

15. Every coil is to be weighed separately, and the weight stamped on a metallic label, which is to be firmly fixed to the inner part of each coil by a piece of iron wire of not less size than 60 lbs. per mile. Each coil will likewise be sealed with a leaden disc, which will bear the examination mark. All labels and discs to be provided by the Contractor, free of expense. The form of both to be decided on by the Supervisor of Telegraph Stores.

16. The wire to be coiled carefully, so as to resemble newly drawn wire as much as possible, and each coil to be securely bound with four separate binders of galvanized iron wire of strong quality, and of not less size than 150 lbs., nor greater than 600 lbs. per mile. For the sake of distinction the binders of Cable Wires are to be ungalvanized.

17. Proper accommodation must be provided by the Contractor for the storage of approved, and the distinct and safe custody of, rejected wire, until finally disposed of, under the control of the Supervisor of Telegraph Stores, and the Examiners of the Department.

GERALD C. TALBOT,

India Office, *Director General of Stores.*

LINE WIRE.

TABLE of Weights and Tests for Standard Sizes.

Nominal Size.	Diameter.			Weight per mile.			Weight of 10 Feet.			Tests for Strength and Ductility.										Weight of each Coil.	
	Required.	Allowed.		Required.	Allowed.		Required.	Allowed.		To bear	Twists. *	To bear	Twists. *	To bear	Twists. *	To bear	Twists. *	To bear	Twists. *	Mini-mum.	Maxi-mum.
		Mini-mum.	Maxi-mum.		Mini-mum.	Maxi-mum.		Mini-mum.	Maxi-mum												
	Inches	Inches	Inches	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.		Lbs.		Lbs.		Lbs.		Lbs.		Lbs.	Lbs.
3½	· 47	·251	·258	900	886·5	913·5	1·704	1·682	1·726	2,775	14	2,850	13	2,925	12	3,000	11	3,076	10	95	105
4½	·2325	·229	·236	750	738·7	761.3	1·420	1·399	1·441	2,312	15	2,375	14	2,437	13	2,500	12	2,562	11	95	105
5	·2205	·217	·223	675	665	685	1·278	1·260	1·296	2,081	16	2,137	15	2,193	14	2,250	13	2,306	12	95	105
5½	·2079	·204	·210	600	591	609	1·136	1·119	1·153	1,850	17	1,900	16	1,950	15	2,000	14	2,050	13	95	105
9½	·1470	·144	·150	300	294	306	·568	·556	·680	925	24	950	22	975	21	1,000	19	1,025	18	70	80
12½	·1039	·102	·106	150	147	153	·284	·278	·290	462	17	475	16	487	15	500	14	512	13	70	80
15½	·0735	·072	·075	75	73·5	76·5	·142	·139	·145	208	24	214	23	219	21	225	19	231	18	35	40
16	·0657	·065	·367	60	58.8	61·2	·113	·112	·114	167	26	171	25	176	23	180	22	185	20	35	40

Note.—It will be seen that the following manufacturing margins are allowed, viz.:—For wire weighing 600 lbs. per mile and upwards, 1½ per cent. in size, and for wire of less than 600 lbs., 2 per cent. in size. The tests for tensile strength and ductility are strictly in proportion to the size of the wire, except in the two smaller sizes, where the tensile strength is reduced 10 per cent. on account of the disproportionate amount of zinc.

In sizes above 150 lbs. per mile the vices will be six inches apart, but in those of 150 lbs. or less, they will be three inches apart, and the number of twists entered are calculated proportionately.

CABLE WIRE.

TABLE of Weights and Tests for Standard Sizes.

Nominal Size.	Required Weight per Mile.	Corresponding Tests for Strength and Ductility.										Weight of each Coil.	
		To bear	Twists in 6″.	To bear	Twists in 6″.	To bear	Twists in 6″.	To bear	Twists in 6″.	To bear	Twists in ″6.	Maximum.	Minimum.
		Lbs.		Lbs.		Lbs.		Lbs.		Lbs.		Lbs.	Lbs.
3½	900	3,000	10	3,075	9	3,150	8	3,225	7	3,300	6	95	105
4½	750	2,500	11	2,562	10	2,625	9	2,687	8	2,750		95	105
5½	600	2,000	12	2,050	11	2,100	10	2,150	9	2,200	8	95	105

NOTE I.—In order that the wire may be readily recognised as cable wire, the binders of the coils are to be ungalvanised. See Para. 16.

NOTE. II.—No joints will be allowed in Cable Wire. See Para. 11.

Colonel Robinson, R.E., had little to add to Capt. Mallock's description of the testing. The advantage of working by weights was obvious. In drawing wire of long lengths, it was impossible to retain the same diameter throughout, but the resistance of the wire depended not upon the maximum and minimum ductility, but upon the quality of iron in a given length the current had to pass through. If they worked by weight they got the resistance constant, that was, a little diminution of ductility in one place was compensated by extra thickness in another place. In making their arrangements for transport, it was convenient to have a round sum and also the lengths. For instance, say six parts averaged so many yards, if they had a constant quantity for those six parts, they had no broken quantities. He mentioned this for what it was worth. He had had no great experience personally of testing the ductility of wire under tension; but it appeared to him if they had tensile strain, combined with the torsion test, they got all that was necessary for securing the strength required, combined with a tolerably ductile wire.

Mr. A. Bell suggested that some inaccuracies in the bursting test might arise from the slipping on the clamps, but a certain elongation took place between the clamps, longer than the correct one. To avoid that he put a mark upon the wire, at a point which could not lead to error. The elongation of the wire reduced the diameter of course, and the clamp might become loose and not give a correct result at all.

Mr. Ayrton said the hole in the clamp was so small that there was little chance of slipping. The screw he had described was seldom used, and fixed the wire without it. Looking to the plan of testing described by Capt. Mallock on the board, he was afraid the method of testing in that way by weight was only applicable to soft wire, and was of little use in the case of the hard wire, which they put into cables. The plan was a good one, no doubt, for the soft wire of land telegraphs, but not for that of submarine cables. He would be glad to hear from Capt. Mallock why he used dead weights in preference to the hydraulic machine in testing the wire for the Great Western Telegraph Cable — the hydraulic machine with which he was acquainted—and why he thought it would not give more accurate results than using dead weights. It was difficult to add dead weights with such nicety as to have no jerks, and for that reason he preferred the hydraulic machine to the addition of weights even by the slow process of using that for the purpose.

Colonel Robinson, R.E., said there was one question of great importance in testing wire; that was the practice of killing the wire. At present he had not availed himself of killed wire. He referred to the principle of Mr. Cromwell Varley passing the wire over a large diameter and then over a small one, taking out all the little irregularities.

Capt. Mallock remarked, with regard to killing wire, he tried some experiments in 1872; he took some wire and put it through the Post Office machine. It stretched; and then he took wire off the same coil, of the same size, before it was killed, and put it through the same machine, and he found it broke at the same weight, that was to say, it took a certain number of pounds to kill it, and so many more pounds to break it. But if he took killed wire, which had been stretched, and took the diameter of that, and measured it against wire not killed and not stretched of the same diameter, then the wire not stretched would come finally to a greater breaking strain, the killed wire being harder for the same diameter; that was to say, in stretching it, he reduced its diameter; but when he took the same wire and first killed it, and then tested it against what was not stretched, the result was the same. They had a plan of killing the wire in India which it might be interesting to state, and which he might say had answered; that was, to kill it after it was put on the posts, so that they got the ductile wire to lay out, and then they put it on the posts, and killed it; whereas, if they killed the wire first, they got hard wire full of kinks, and they must put a good strain on that to get the kinks out. The way they coiled the wire on the posts was similar to the method of tying the reeds together in a Pan's pipe. Supposing they were going to stretch a wire over 7 posts, in putting it up, it would be on No. 1 off No. 2, on No. 3 off No. 4, and fastened to No. 7, and the 5th post would be the measuring post. A man was placed there with a bamboo rod, and exactly so many inches between the bracket, in proportion to the dip he wanted to give, he put a chalk mark. He believed it was 24 inches per mile: then he had a measure 88 inches long, and below each bracket he took his 88 inches and made a measure on the post, and when this wire got to that it was in its proper place; but to kill it, it was lifted to half the distance, that was to 44 inches, so that having half the dip, they had half the working strain. All the wire was put up and the dips were calculated at 1/4th the working

strain of the wire. The killing strain being brought up to half this dip of the measuring post, was half breaking strain, and that plan of killing the wire on the line instead of doing it before it was taken out had another advantage, and that was, it tested every joint of the wire, and they put on every joint of the wire twice its working strain. Mr. Ayrton asked a question about the hydraulic machine. His experience with that machine was chiefly in 1864, and he found there was so much friction that he could not depend upon it; and the machine he preferred was the lever, with an arrangement for winding up the wire to take out the stretch. There was a clause in the specification that the wire was to be tested by direct weights, and not by lever: it was weighted with their own weights, and there was no question of friction, or the machine getting out of order. That clause was put in by Mr. Moxon's approval, and as that had run over thousands of tests, he believed there was no harm in it.

Mr. Ayrton said in the hydraulic machine he had described there was absolutely no friction—not so much as there was in a simple lever. The advantage was so great that though Mr. Johnson was of the same opinion as to the hydraulic machine that Capt. Mallock was, he was induced to go to Manchester to see the machine, he so much approved of it that he ordered one and intended to use it for testing his wire. It was next to impossible for it to get out of order.

Mr. W. H. Preece said the paper was intended to deal only with the question of the ductility of the wire, and the testing applied to measure that property, but the discussion had branched into such a wide range of the subject that he would go back a little bit and trace, as well as he could, the history of the wire now used in England and the tests applied to it. In the early days of telegraphy when expense was not of much consideration, the wire used was the best charcoal wire that could be produced, and that wire fulfilled all the requirements of telegraph wire in strength and ductility and all other properties, but its expense was so great that when telegraphy came to be a commercial undertaking the early telegraphists had to look round to see if they could not find other material to answer the purpose. The result was the wire known in the trade as "best best" was introduced. The early specimens of wire used were very indifferent, flaws, welds, and other things caused ruptures when the wire was erected. It occurred to the minds of those who erected the lines that some test of the wire was necessary; they saw that while

it was necessary to obtain wire to give a certain tensile strength, it was necessary at the same time that the wire should be soft and pliable and should possess ductility, the necessity for ductility being this, it frequently happened that trees and signal-posts fell across the wires and if they were not soft and pliable they would be broken. In those days the joints were twisted joints and that required that the wire should be ductile; they found if the wire was sufficiently soft and ductile it had also sufficient tensile strength for their purpose, for, as a rule, in practice the tensile strength applied to wire was very small. It was rare in England that the spans of wire were so long as to require the use of homogeneous wire or steel wire, but as a rule the spans were so short and the strain was so small that great tensile strength was not necessary. It was unnecessary that the wire should be highly annealed and soft, and if they could get a wire which under test elongated 18 per cent. it fulfilled all the requirements of English telegraphy, and they had proof of the value of the wire from this simple fact, that when new line wires were erected and frost came on they were not broken down. Formerly every frost invariably was followed by hundreds of breaks all over the country. There was a curious instance of that at the time of the transfer. One railway company erected wires over the system for itself and they did it with their own wire which was not put under the tests which the Post Office required; and the result was in the whole of the telegraph districts, which were largely extended in 1870, of all the thousands of miles of wires erected, none of them suffered so much from broken wires as the line which was constructed with wire which had not undergone testing; all the wires that were tested withstood the frost completely. Now, in practice the wire was never subject to twisting in England, it was bent and rebent in making joints and terminals, and in testing wire it should be tested to endure that kind of strain to which it was likely to be subjected. Hence, theoretically bending backwards and forwards in the way described by Mr. Ayrton was as good a test as could be applied, but unfortunately this test could only be applied to a small portion of the wire, and as Mr. Culley's paper was directed to point out the inutility of applying tests to small lengths of 10 inches, it was absurd to introduce tests to be applied to shorter lengths than that. Mr. Culley showed that 10 inches was not enough and that probably in future he would apply the tests to lengths of 100 inches, but the 10 inch lengths enabled the calculation of the

percentage of elongation to be taken, for there was a scale on which they could read off the percentage of increase. The machine at Mr. Johnson's and other places not only registered the elongation of the wire, but the tensile strength. The process gone through in testing was this—First, it was gauged for diameter, specimen pieces were then weighed to see how far they were in compliance with the contract, it was then tested simultaneously for tensile strength and ductility by the machine before mentioned. The tensile strength was in reality that which they simply specified if the wire showed an elongation of 18 per cent. of its length. Colonel Robinson had asked for some information respecting the method adopted for killing the wire. The term "killing" was introduced he did not know by whom, or when. The wires at one time were erected full of kinks and bends, and flaws; and it was found by some one that if the wire were stretched on the line in long lengths, these would be taken out, it became straight and limp, and pliant, and here arose the term "killed." But there was an objection to killing wire. The limit of elasticity of No. 8 wire, is 450 lbs., and from that point it could be stretched till it broke, but it was impossible by the rough method they had on their lines—and not so rough—but still rough method in India: it was impossible to reach the proper limits of elasticity, which required that the wire should be just stretched, and no more. When they put a wire through a machine which elongated 2 per cent. of its length, they attained the limit of elasticity they wanted. It did not injure the wire, but it did what they wanted, inasmuch as it enabled them to detect all those flaws and points of danger which, if they remained, would break down the circuit. Hence they killed the wire, and by specifying that in the manufacture it should pass through a machine which elongated 2 per cent. of its length, they got the application of a reliable test to the wire. It was true the tests when applied were dependent upon the skill of the manipulator, and therefore both in the Post Office and in the India Department they employed skilled inspectors, whose duty it was to apply these tests. No doubt it would be a great improvement if a machine were introduced which would dispense with this mechanical skill, and while Captain Mallock had broached the idea which, as applied to this particular test, would give a test independently of mechanical skill, he, Mr. Preece, was afraid they would not apply it, because it put a wire to a test which they did not want.

What they wanted was, a test simultaneously of ductility and tensile strength, and if this could be done independently of the skill of the manipulator, it would no doubt be a great improvement. He was an advocate of the total abolition of the *Birmingham* wire gauge, there could not be a more ridiculous thing. It meant nothing, and was introduced into telegraph phraseology as a rule which was useful, but not scientific, and ought to be replaced by something more creditable to the present age. Captain Mallock started a good notion when he proposed that the wire itself, by whatever name it might be known should be based upon some scientific basis. Captain Mallock suggested that that basis should be weight, let the wires be some multiple of some unit of weight. By that means they would get what they had not now, viz., a measure of the resistance of the wire, and the resistance of the wire was most essential. With regard to iron wire, they knew nothing of the resistance till the circuit was erected and the tests were applied. If they had a system based on weight, they would have a unit to start with, and something by which to compare the qualities of wire and its resistance. By giving the weight they would know what the resistance was, and they would have some method that would enable them to estimate better the electrical qualities of the wire than they could at present Such improvements had been introduced into wire that they ought to be ashamed of themselves for not having adopted some more methodical method for measuring the resistance of iron wire. Mr. Culley had shown that the test of ductility on short lengths was not reliable, and others, no doubt, had found it to be so. He (Mr. Preece) was led into a curious error from this circumstance —Wire supplied from Messrs. Johnson's manufactory at Manchester, which gave the specified ductility—that was an elongation of 18 per cent.—was sent down to his store place at Bishopstoke, and laid there for some months. He applied careful tests to that wire, and he found, instead of stretching 18 per cent., a very large series of experiments showed that it stretched only 6 or 8 per cent. The conclusion he came to was that the wire became injured in respect of ductility by exposure to the atmosphere. It elongated 18 per cent. when it was first coiled, and when it was sent to Southampton, only 8 per cent. The error he now found to be due to the fact that he tested it not in lengths of 10 inches but of 16 feet, and as Mr. Culley had shown, in some cases the elongation in 100 yards lengths was really only about 5 per cent. though in actual practice at the works it elongated 18 per

cent. He thought this fact was important in itself and was worthy the attention of those who tested large quantities of wire.

The CHAIRMAN, in bringing this discussion to a close, said he begged to propose a vote of thanks to Mr. King and Mr. Culley for the interesting papers they had presented, and also to Mr. Preece for the able way in which he had read and assisted in the discussion of the latter paper. They must all regret the absence of Mr. Culley; but it must be admitted he had committed his paper to very excellent hands. These were both most interesting subjects. Mr. King's paper had given an idea of supplying a want which it would be very interesting to supply. The question of the tensile strength and testing of telegraph wires was a very important one. He could say as far as he was concerned he had been interested, and had received instruction from what he had heard this evening. He therefore begged to propose a vote of thanks to Mr. King and Mr. Culley for their papers, and also to Mr. Preece for the manner in which he had read the latter paper.

The following Candidates were balloted for and declared duly elected:—

AS FOREIGN MEMBERS:—

F. Von Hefner Alteneck	Berlin.
Don Waldo Aguayo	Santiago.
General Thomas Eckert	New York.
Dr. Ad. Lasard	Berlin.
Don Ramon Pias	Santiago.
George Prescott	New York.
Don Diego Torres	Santiago.
Don A. V. Ugarto	Santiago.

AS MEMBER:—

Juland Danvers	India Office.

AS ASSOCIATES:—

Alex. Adams	Postal Telegraphs, Telegraph Street.
A. Collins	4, Queen's Square, Westminster.
Ernest Cook	101, Cannon Street.
William Durran	Alexandria.
Alfred Frost	5, Westminster Chambers.
S. E. Jones	24, City Road.
Alex. Mackie	Warrington.
Thomas Newsam	Alexandria.
J. G. Parker	St. Thomas, West Indies.
George West	Alexandria.

The Meeting then adjourned.

The Seventeenth Ordinary General Meeting was held on April 23rd, 1873, Mr. LATIMER CLARK, Vice-President, in the Chair.

The first paper read was "ON THE BLOCK SYSTEM OF WORKING RAILWAYS," by W. H. PREECE (Member).

ON THE BLOCK SYSTEM OF WORKING ON RAILWAYS.

By W. H. PREECE, M. Inst. C.E. & S.T.E.

THE Railway System of this country is a vast working machine of the most complicated character, which is as yet very far removed from the summit of the Engineer's ambition—*perfection*, but which is certainly approaching that point. Starting from a stage coach planted on flanged wheels, drawn by a locomotive, directed by flags by day and burning tarred rope by night, conveying but few passengers at long intervals of time, it has become what we now see it in the underground arteries of London, and in the iron veins of the country, governed by the most elaborate machinery, and guided by the most subtle of the physical forces, the safest of all means of locomotion, and the highway for all the lives, the wealth, and the goods of the people. At every moment of the day and night, trains of all characters are rushing with mad velocity in all directions, but with well-nigh perfect safety, and with marvellous precision.

While the carefully prepared statistics of the Board of Trade show that the traffic is increasing with immense rapidity, the average number of fatal accidents which occur is nearly stationary. In the three years ending 1849 the average number of passengers killed by causes beyond their own control was 12, being 1 in 4,782,188 passenger journeys made; while in 1871 the number killed was the same, but the proportion to passenger journeys was 1 in 31,250,000! We have, thus, powerful evidence of improved working and of the beneficial effect of the process of scientific thought, which is the application of the lessons taught by past experience to different circumstances.

The reports of the Inspecting Officers of the Board of Trade upon

accidents which occur, are not only interesting as narratives, but instructive as to the different modes of working on different lines. They are the records of past experience. They show exactly where the shoe pinches. They favour the application of the law of average to bring out the defects or merits of variations in railway working. Dangerous modes of working are frequently carried on for long periods without accident, but inevitably they break down at last. The Board of Trade Inspectors publish these accidents, and bring to the knowledge of the many what would otherwise have been the experience of only the few. The lessons taught by experience are thus promulgated, discussion is invited, and the process of pure scientific reasoning applied. These reports furnish experimental data to form the basis of a true science—the science of railway working. There can be no doubt that the great improvement that has been made in the working of our railways is due to this action of the Board of Trade, and this beneficial effect would have been materially hastened if the Press had given greater publication to these valuable records.

The number of accidents which have been investigated during the past two years have been:—1870, 131; 1871, *171.

A careful examination of 138 accidents showed that the causes which led to them might be summarised thus:—

1.	Defective permanent way	18 per cent.
2.	Defective rolling stock	13 ,,
3.	Defective human machinery	41 ,,
4.	Defective signalling arrangements	28 ,,

Of these accidents 56 per cent. were the effect of collision, and were due to the last two causes.

Taking the year 1871, and the different Railway Companies having termini in London, we get the following table:—

Railway Co.	No. of Accidents.	Mileage of line.	Mileage per accident.
South Western	0	657	
South Eastern	0	327	
Brighton	1	351	351
L. C. and Dover	1	139	139
Great Northern	5	633	126·6
Great Western	11	1387	126·
Great Eastern	11	874	79·4
North Western	21	1507	71·8
Midland	15	972	64·8

* The increase is probably due to the fact that the new Act came into force in 1871, rendering it compulsory on Railway Companies to report accidents.

What is it that causes such a wide difference in the safety of trains moving upon the lines at the head of the list, as compared with those at the bottom of the list? Is it that the elements of accident are more abundant in the latter than in the former, that the traffic is more crowded, that there is greater variation in the character of the trains, and that the speed is higher? Or is it that the systems are more lax, and that the accommodation is more defective? I cannot answer these questions, but I will mention this fact, that upon the Southern lines the Block System is almost entirely in use, while upon the latter it is only partially so. Thus in 1872:—

Railway Co.	Worked by Block. Mileage. m.	ch.	Total Mileage.
Midland	422	40	972
North Western	241	49	1507
South Eastern	327	0	327
South Western	325	70	657

Of the 16,000 miles of railway in the United Kingdom, 4,515 are worked on the block system, viz:—

England and Wales	3,689 miles.
Scotland	790 "
Ireland	36 "

Now, what is the *Block System?* It is that system by which trains are kept apart upon the same line of rails by a certain and invariable interval of *space,* instead of by an uncertain and variable interval of *time.* The term *Block* is an unfortunate selection. It was introduced through the practice of "blocking" or pinning the telegraph needle over, in the earliest instruments used to work the system. The "space" system in opposition to that of "time," would have been more accurate; but the word "block" has now become so thoroughly rooted in railway language that it would be difficult to supplant it.

The practice under the time system is, to exhibit the danger signal for five minutes, and the caution signal for five minutes more, after a train or engine has been despatched from or passed any station, junction, level-crossing, or siding. Trains are thus kept apart by fixed periods of five minutes, and if the caution signals were properly regarded, by an interval of time even longer than that. The safety of the train is entirely the responsibility of the driver. Immunity from accident is dependent upon his keeping a clear look out. If engines ran at regular and fixed speeds, if time tables could be adhered to, if the line be not crowded with traffic, if the driver could always ensure a good view

before him, if signals were near together and they were properly regarded, then a rigid interval of time might be maintained between following trains; but none of these elements of safety are constant. Fast expresses follow slow goods trains, now through a thick fog, now up a wet incline, at one moment in bright sunshine, at the next in a thick snowstorm, creeping mineral trains break down in a long interval between two stations, passengers rush in at the very last minute, detain the train, and prevent the time tables from being adhered to, trains are so frequent at some places that the five minutes interval cannot be adhered to, obstructions to view arise from curves or cuttings, or from atmospheric causes, long lengths of line are unprotected by any signal at all, and signals themselves are too frequently neglected. Hence, the system is brimful of elements of danger, and the inexorable logic of facts has shown that the time interval is illusory and the system unsafe.

But when trains, however rapidly or slowly they may be running, however much punctuality has been infringed, however crowded with traffic the line may be, are invariably kept apart by an interval of one or two miles, collision between them becomes impossible. This is the *Block system*, which has, very improperly, been divided into two classes—the *absolute* and the *permissive*. The former is the block system proper, the latter is not a "block" system at all, but a system introduced by the London and North Western Railway Company at the suggestion of Mr. Edwin Clark, not to secure the safety of their trains, but to increase the capacity of their line for the transmission of their increasing enormous traffic. It is, doubtless, an improvement on the time system, but it bears little affinity to the block, and should certainly not be included in the same category.

I wish to draw a broad distinction between the block system as an abstract principle, and the means of carrying out that principle. The two are very much confused. We hear of Tyer's, of Walker's, of Spagnoletti's, and of other block systems. They are not block systems; they are simply instruments devised to carry out the electrical portion of the block principle.

The carrying out of the block system means a great deal more than the erection of wires and the fixture of instruments. It means the entire re-arrangement of the rules and regulations of the traffic-working, a complete reorganization of the signals, the construction of fresh siding accommodation, the education of the staff, the transference of much responsibility from the drivers to the signalmen, and the supply of proper

cabin accommodation. Moreover, the working of the block is not necessarily an electrical question. If the sections of line be very short, it could be worked by mechanical or pneumatic power, and where sections are long, and trains are few, it could be worked with a "staff" which is practically working with a single engine.

The time system having been proved by practice to be inefficient, and the block system, having shown itself to be a safer principle upon which railways should be worked, how is it that its introduction has been so slow? People, as a rule, are very sceptical of any new invention until it has been practically established for a long period, and particularly so when it is based upon principles which are not understood. It took the public a long time to get used to railways, and a much longer time for railway authorities to get used to the telegraph. The telegraph was but lightly regarded. It was looked upon as a costly auxiliary, bringing in no returns. How far recent changes have modified this view I cannot say. I speak of matters as they were before the transfer.* The accidents it has prevented, the lives it has saved are never known. It is usually the last thing thought of in the construction of a line, it is the first thing flown to in moments of difficulty and danger. It is a willing slave, always ready, and rarely wanting. But its principles are not understood, and the application of electricity to signalling purposes is still less comprehended. Hence, in addition to scepticism, we have ignorance of principles delaying the introduction of the block.

But its introduction is also costly, both in its first outlay and its maintenance.

For instance—

Railway Co.		First Outlay.		Annual Maintenance.
South-Western	...	£125	...	£60 per mile.
London, Chatham and Dover	...	174	...	60 ,,
Midland	...	52	...	29 ,,

This cost is not due to the electrical appliances, which form but a small item of the total, but to the signal boxes with their interlocking apparatus, and the various accessories to which allusion has been made as being part of the block system.

A certain amount of prejudice has also existed against its working. It is said to introduce delay into the working of the traffic, and to be objectionable because it removes the entire responsibility from the driver to the signalman. Experience—the true test of such objections—has shown

* The transfer of the Telegraphs to the Government.

that, under proper regulations, there is not only no delay, but absolute expedition in the working of trains under the block system, and the capacity of the line for the conveyance of trains is largely increased by its means; and as regards the responsibility of the driver, it has, if anything, been increased, for its correct working depends essentially upon his keeping a good look-out, and upon paying proper regard to the signals. Moreover, it has instilled into the driver an amount of confidence which he never felt before, and has thus checked that daring recklessness that was at one time so prevalent. Thus prejudice, cost, and indifference have led to the neglect of the system. These objections are, however, rapidly dying out, and most of our great Railway Companies are now largely extending the block.

Should the system be made compulsory upon Railway Companies by legislative action?

The Board of Trade already insist upon its introduction on many new lines before they are opened to the public; why should not the same principle be introduced on all old lines? Parliament has compelled the railway companies to supply smoking carriages, and to establish intercommunication in trains; why not also compel the introduction of the block? It is clearly undesirable to interfere in any way with the responsibility of the railway authorities. It is for them to regulate their traffic, and to select their machinery. But the block system is, as I have shown, an abstract principle, the working details of which can be worked out in any way. Its enforcement can in no way lessen the responsibility of the railway company, who are entirely left to select that system which they consider best. But, on the other hand, it is not the only panacea for the prevention of collision; locked levers, the proper distribution of break power, uniform signalling, &c., have to be considered, and if Parliament enforces one principle, why should it not enforce all? I answer, Parliament has already legislated; and if the block system be made compulsory, all the other improvements must follow the block, since I have shown them to be a part and parcel of that system. But railway companies are rapidly introducing the block without compulsion; why make it obligatory?

Improvements, the lessons of experience, are not applied quickly in this country. It takes a long time to become "fairly satisfied" of the benefit of any improved change. Under our commercial system, safety and convenience are often sacrificed to profit, and though a manager may greatly desire a change, a board of directors may hesitate to spend a large sum upon an improvement which will not add to their dividends

and which they do not clearly understand. But if it be made compulsory there can be no excuse. The necessary sums must be voted, and the system must be carried out. I am inclined to think that many railway managers would be only too glad to find their boards forced by Parliament to adopt a system which must tend considerably to reduce their own responsibility. It is stated that publication and discussion would be as effective as compulsion. Railway Companies are very sensitive to public opinion. The public can exercise great power and control in the working of railways. We have an instance of the power of publication in securing good work in the Post Office, which as Mr. Scudamore said, "is not popular because it is efficient, it is efficient because it is popular." But the public can have no insight into the internal working of a railway system. It can only control comfort, speed, and punctuality. And the public itself is not blameless, for it will encourage reckless competition, it will have high speed, and it will be late.

If the Block system be made compulsory upon all railways, what means are to be adopted to carry it out?

There is no difficulty in selecting fit and proper electrical and mechanical appliances to work it; many workers have been labouring in separate grooves, and efficiency has been attained, not by joint and uniform management, but by emulation and competition. Of the various railways radiating from the metropolis, no two are worked with the same instruments; but all, by a process of natural selection, are approaching the same goal. The "principle of the survival of the fittest" will doubtless eventually determine the proper method to be adopted, for it seems most desirable in the internal electrical signals, as in the external visual signals, that uniformity should be secured. I am far from advocating the Block system as a panacea for every ill inherent to railway working. Interlocking gear, the proper distribution of break power, effective intercommunication in trains, sound permanent way, more perfect rolling stock, the regulation of speed, are all points upon which accidents hinge, and which are as essential to safe working and as worthy of legislation as the block system. But I contend that they follow in the wake of the block system; and that, if that system be made compulsory as a principle, all the other improvements must follow without transferring any responsibility from the railway authorities to the Board of Trade.

The following paper, on the same subject, was read by CAPTAIN MALLOCK :—

I WAS lately ordered by the Government of India to report on the use of Telegraphs in connection with Railways in England—a subject of which the most important portion is the Block System.

My report, from want of time, is as yet unfinished, more especially as regards the descriptions of the working details of the instruments, but the most essential portion of it has been presented to Government.

As, however, I may perhaps have had pre-conceived notions in some one direction, or from inexperience have taken an exaggerated view of some points, it is well both for the sake of the Indian Government and and the Indian Railway Companies on the one hand, and where I have described instruments, for the interests of inventors on the other, that my views may be disputed and discussed. Whilst on general grounds, having taken a great trouble to collect all the procurable information, and ascertain what is really desired, the facts I have put together may perhaps be useful to those who, like myself, are trying to find out the best system, and also to Telegraph Engineers, who in adding to their present invention, or devising a new one, may be reminded of all the many requirements to make the system perfect. I have therefore received permission to read extracts from my report at this meeting, and I believe that whether I can prove, or any of you present will disprove certain rules that I give, we shall be able to determine on a certain number of desiderata, when it may be reasonably assumed that any system which at the present time meets the most of these will be the best for our purpose.

Keeping this object in view, we may as well omit any discussion as to priority of invention of the whole or any part of the systems at present in use.

Mr. Preece has told you already of the difference between Absolute and Permissive Block, there is, therefore, no need for me to describe them; but it will not be out of place for me to read here some extracts of a Report of Captain Tyler's, on an accident occurring on the South Wales Section of the Great Western Railway, on 5th November, 1868, in which a cattle train was ran into by a mail train.

Captain Tyler says :—

" The unfortunate drovers suffered in this case from being placed in a
" carriage at the tail of the cattle train, an arrangement which has proved
" fatal to others of their class in a similar manner on previous occasions,
" and which is therefore undesirable."

" It is adopted for convenience in making the break carriage in that " position serve both for guard and for drovers, but the drovers might be " carried more safely in the middle or near the leading end of the train."

" The principal object in view, should, however, be to prevent collisions " from occurring, rather than to enable the drovers to sustain such collisions " from a position of greater safety. And looking at the subject in this " light, the more important lesson which the present, in common with the " more fatal accident at Abergele, and with many other previous cases of " collision tends to enforce, is the necessity for a more extensive use of the " telegraph, for ensuring a clear course and a line safe from dangers and " obstructions in the path of these fast trains."

" A mere interval of time, whether it be ten minutes or twenty minutes, " or any other given quantity, affords no true margin of safety. * * * " * * * An interval of space is the only true remedy for such a " state of things, and it matters not what the distance between the trains " may be, whether 4 miles, or 2 miles, or ½ mile, so long as it is strictly " preserved. * * * In this particular instance, an interval of time of " no less than 22 minutes proved to be of no avail."

In either case, whether it be Absolute or Permissive, the Block system, as chiefly used in England, is for use on double lines of railway, and may be distinguished generally under the name of " The Non-following Block," in contradistinction to "The Non-meeting Block," which is essentially necessary on single lines. This Non-meeting Block is preferred by the inspecting officers of the Board of Trade, to be carried out under the train-staff and ticket system; that is to say, that each section of the line has a train-staff, and, as this is carried on the engine, it necessarily follows, theoretically, that no other engine can meet it. When, however, it is necessary for more than one train to travel in the same direction, the last of the set of trains carries the train-staff, whilst others are allowed to proceed under a system of tickets. It is evident, however, from the accidents which have occurred, and quoted below,* that, from various causes intervening, the train-staff, even if not inoperative in the event of irregular trains running, gets set aside, and at a discussion on this

* (1) Collision on the Cambrian Railway, 1st November, 1869. (2) Collision on the North Eastern Railway, 15th January, 1870. (3) Collision on the London and South-Western Railway, 22nd February, 1870. (4) Collision on the Cockermouth, Keswick and Penrith Railway, 2nd September, 1870. (5) Collision on the Isle of Wight Railway 15th April, 1871.

system, at the Institute of Civil Engineers, in January, 1863, the following remarks were made, amongst others :—

1. Mr. Hemans "had found, even when running only four trains per "day in each direction, that it was next to impossible to repair defects on "the line without working in the middle of the night, owing to the diffi- "culty of getting the "train-staff" from tne passenger trains, for the "use of the ballast trains, or even to run extra trains at times of fairs and "markets."

2. Mr. Preece "stated it was upon single lines that the adoption of "the telegraph was most urgently needed. It frequently happened on the "Epsom and Leatherhead line, that a man had to be sent on horseback or "by carriage to the other end of the line to obtain the staff."

3. Mr. Hawkshaw, President, said "he placed a high value on the "opinions of Captain Huish, who had such long experience on this "subject; but he could not agree with him on one point. Theoretically, "no doubt, the train-staff led to greater safety than any other mode of "working. But persons might be preserved from railway accidents by not "going upon a railway at all; and the train-staff system was an approach "to doing away with railway travelling. As a system, it simply meant "that, provided a Company confined itself to one staff, it would also "limit itself to one locomotive; and so long as one locomotive only ran "upon the line, there could not be any collision. But it was obvious "that, under that system, trains could not be started from the same end "of the line at short intervals of time; as when the staff was at the other "end, it must be brought back, and this necessarily involved delay. He "considered the train-staff system was wholly inapplicable to any railway "on which the traffic was large."

There are various methods of carrying out the Block system, any one of which, looked at by itself, is apparently perfect, but to choose between them it is requisite to know what is really required. No rules on the subject have been issued by the Board of Trade, and no special report on it alone has been made by the Inspecting Officers, but their Reports on railway accidents teem with references to it.

It is reasonable to assume that a knowledge of the requirements of a system may be gained from results of accidents which have occurred either through the want of a Block System, bad management, or defects of a system in use. To gain this knowledge, and place it at the disposal of the Government of India, in a collected form, I procured copies of the Inspecting Officer's Reports of Railway Accidents for the last ten years

separated from the bulk those connected with telegraphs, and sorted these again into classes. It would be impossible to read the one-hundredth part of these now, nor is it necessary; but I have brought them with me so as to answer any question on the subject, or to show to any one who may care to refer to them. From each class or from statistics a rule or axiom may be plainly deduced. These are as follows:—

1. The block system entails no loss of time, but, on the contrary, enables a far greater extent of traffic to be carried on than can possibly be done without it.
2. The block system is essential to the safe working of the line.
3. The system of the permissive block is most dangerous.
4. It is, however, no use to establish the block system if either it be rendered impossible to work it, owing to the section being too short for a train at full speed to stop, or if a slack method of working, and disobedience to orders be permitted.
5. It is not only necessary to guard the main line by the block system, but any station at which shunting is going on, or may go on, must be looked upon as a section to be guarded by block signals in connection with the distant semaphore.
6. A private code of signals between the men must not be allowed.
7. Whatever system of block instruments be introduced, a train register book is wanted.
8. It is desirable that on special dangerous sections, the train should report back its own arrival electrically.
9. Besides the block instruments proper, a talking telegraph is desired to put into communication any section, on which there is likely to be shunting, with the next blocking station, and these talking instruments must not be interposed in circuit with others, but each station must be worked as a terminal.
10. At exceptionally dangerous places, the single wire system is insufficient.
11. At exceptional sections, the block instruments should interlock with the semaphore levers.
12. "Line clear" should not be signalled back until the tail-lights or tail-board of a train have been seen, so that the man is sure that the whole train has passed him, and none of the carriages are left on the road.
13. For a single line, even when the train-staff demanded by the Board

of Trade is in use, it has proved inoperative without the assistance of the telegraph.

14. At all stations where the distant semaphores or lamps are out of sight of the signal cabin, electric repeaters are required to show that the semaphore is up or down, or the lamp burning or out.
15. The block system is as necessary with light as with heavy traffic.
16. Where the bell code is in use, a large number of beats are liable to lead to confusion.
17. Needle instruments which can be used for talking are objectionable for block instruments.
18. It shall be impossible after the instrument has shown "line blocked" for it again to show "line clear," unless both the signal-men at each end of the line are concerned in making the signal.
19. On Tyer's and Walker's instruments, the motion required for moving the plungers or buttons to give different signals of "line clear," and "line blocked," are so exceedingly alike that in a hurry a mistake occurs.
20. At the junction of two lines, the block instruments should interlock (on the principle of Saxby and Farmer's interlocking semaphore levers), so that "line clear" cannot be telegraphed to both sections at the same time.
21. Exceedingly simple instruments are required, so that a man with little practice may readily learn them.
22. The block telegraph and signals should always be worked in unison to announce danger or line clear, as the case may be; and, therefore, the method of working the telegraph should approximate as much as possible to the method of working the out-door semaphores.

And the following rules, although not specially referred to by the inspecting officers, may, as will be seen from studying the working details, be added:—

23. A single wire system, working with temporary currents, cannot be considered as perfect, if lightning can reverse the signal, or demagnetize the instrument.
24. The receiver of a signal must not be able to alter the index of *incoming signals* at his station.

The first 16 of these rules or axioms are evidently applicable to any

system or class of instrument—the last 8 assist us to determine the best.

There are Seven different classes of instruments* now used for the Block System, viz.:—

1. A double needle instrument combined with a bell, introduced by Edwin Clark, on the London and North-Western Railway, in the year 1854.
2. Electro-magnetic semaphores combined with bells, introduced by Preece in 1862.
3. A variation of Clark's needles and bells, introduced by Spagnoletti in 1862, caused by attaching discs either to the ordinary needle instrument or to his patented induced needle.
4. Working by bells alone, introduced by Walker, on the South-Eastern Railway, in the year 1851.
5. An electro-magnetic semaphore, combined with bells, introduced by Walker in 1866.
6. Two indices, combined with bell, introduced by Tyer in 1869.
7. A modification of his original instrument by Preece, introduced in 1866, and improved in 1872.

The first three of these systems, which are usually called "three-wire systems," work with a permanent current always on the line, and require one wire for each line of rail, plus one wire for a bell, while the last four, called "one-wire systems," work with temporary currents, and only one wire is required to work both semaphores (or indices), and also a bell, for two lines of rails.

There is no time to describe all the instruments in detail, but, having examined them all, and taking into consideration the rules deduced from the accidents, I believe,

1. That except at certain exceptional places (such as swing bridges, which are made to form part of the circuit for the current, and therefore interrupt it when open) the permanent current, or three-wire system is expensive, and it appears from practice that an enormous traffic can be safely carried on with a one-wire system, *when guarded by a train register book.*
2. A needle instrument of any sort permits of talking, and is dangerous.

* These are the main classes; but there are also variations of them, *e.g.*, the London, Chatham and Dover Railway use Walker's original system on their main line, but guard it with a blocking needle on their suburban sections.

3. Of the one-wire systems. There is no use in contemplating the introduction of Walker's bells without indices, when the addition of these latter is a great safeguard—a little extra expense for the instruments, and at none whatever for the line—and his own opinion is that the bell plus semaphore is unquestionably better than the bell only.

At first sight, these needle instruments seem so cheap, so easy to understand, and easy to repair, that a person not acquainted with the subject would be too apt to prefer them, and, therefore, I now wish to specially point out (which I omitted in my report to the Government of India) what I consider their danger and imperfections, in addition to the facts borne out by the accidents connected with Rule 17.

The theory of working them is that a constant positive current kept on line, by the commutator being pinned over, will show "line blocked," whilst a negative current shows "line clear." In actual practice, however, and in order to avoid consumption of battery materials, "line clear" is only signalled on the train arriving, and then the needle is allowed to fall to zero, the result being that should the index or needle be pinned over to "line blocked," and the line wire break, the needle will fall to zero, showing really "line clear," and thus lead to error. The theory is, that a man ought not to consider the line clear unless he has had a call on the bell, until he is answered, and the needle put over to line clear. But it will be readily seen how easy it is in practice when a man is hurried, or should he in a small station (where there is little traffic), have left his work for a short time, find, on his return, the needle vertical and make a mistake.

Another disadvantage of this needle system is, that the instrument allows a man to use it for a talking one, and although the Board of Trade Inspectors have frequently expressed their opinion that a talking telegraph is most necessary, yet they mean and say only as an adjunct to the block instrument, and not that a talking instrument should be used for it.

Further, and also connected with this power of talking, supposing station A pins his instrument over to "line blocked," and station B for some reason wants to talk with him. B, by moving his commutator, which is in the same circuit as A's needle, will break the circuit, thereby causing A's needle to move. A may therefore either consider the train as in at B, and then remove the block, or may go on talking and thus forget that the block was on the B, and also forget it.

On the London Chatham and Dover line they allow the disadvantages that needle instruments are liable to, on account of lightning reversing the currents, and they get over the difficulty in a somewhat negative manner, inasmuch as "line clear" is shown by the index being momentarily turned over to "line clear," and then allowed to fall to zero. For "line blocked," it is set over to "line blocked." Should a flash of lightning reverse the current, or reverse the magnetism of any needle, the needle would remain at "line clear," which in a negative manner would mean not "line clear" but "line blocked," and then they say "Oh! the needle is standing at *line clear*, so it must mean *line blocked*."

To conclude these remarks on needle instruments, I believe you will find, on comparison, that the lines which are lowest on the comparative list of merit that Mr. Preece has just read, use needle instruments.

There remain then to choose from

Tyer's,
Walker's, } one-wire systems.
Preece's,

All of them good, and, as far as I can see, each possessing certain advantages and disadvantages, which will be better understood from a complete description, but they may be, I believe, summarized as follows:—

1. Tyer's.

Very good workmanship. I can discover no weak point in their construction. Apparently the most sensitive, and calculated to work the longest distances, but liable to have reverse signals given on them by atmospheric electricity.

The plungers which give the signals—

"Train on line," } are so much alike, that a man in a hurry may
"Line clear," } make a mistake.

2. Walker's.

The instrument itself strong; but I think the springs in the commutator worked by the plungers weak in principle, because they get contrary motions given them; that is, a portion of the spring is sometimes on an outside, and sometimes on an inside curve.

It possesses the same disadvantages of possible reverse signals and similar plungers as Tyer's.

9. Preece's.

The commutator, which changes the signals from "train on line" to "line clear," is a miniature semaphore lever, and its motions are the

same as those of the outside semaphore lever. Therefore there is much less chance of a mistake than in the others. Also, as like the semaphore levers, it admits of interlocking, on Saxby and Farmer's principle, "line clear" cannot be given at the same time to two sections which cross each other at a Junction.

By an addition recently made, the signal "line clear" cannot be given unless both sender and receiver are concerned in making it, and therefore an atmospheric current will cause no mistake.

The semaphore portion of the instrument is much like Walker's, and, like it, is not so simple as Tyer's. That is, there are more moving parts, and consequent complication; but there is no doubt of either of them working well. For instance, I was told by the signalmen that of all Preece's instruments at Bishopstoke, which have been in use four years, none have been repaired; whilst at a gate-crossing near, one instrument was sent for repair at the end of seven years. Walker's instruments are apparently quite as durable.

The effect of lightning on Tyer's instrument appears to be this. It will be seen, on examining the instrument in detail, that only one man is required to send a signal, and the receiving index at the distant station will remain at "line clear," or "line blocked," according to whatever signal was last sent. Supposing, now, that a positive current had set the line over to "line blocked," a negative charge of electricity (due to lightning) would set it over to "line clear," where it would remain; a matter of no great moment on a heavily worked line, where the signalman is always in his box; because the current that reverses the signal would also ring a bell, and the man receiving a contradictory code signal would inquire what it meant, when the answer would (by the construction of the commutator) correct the false signal; but, on a line of light traffic, where a signalman may have other duties to perform, and be out of the box when this reverse signal was received, he might, should he not have heard the bell, be led into a mistake on his return. This objection is equally applicable to Walker's instruments.

With regard to the latter, I have no evidence that such has ever happened, but with regard to Tyer's system, several cases occurred with instruments that were being tried on the Great Western line:—

Whilst, however, the system has these disadvantages, it can probably be guarded against by a properly kept train register, and that such is the case is most likely, for Walker's instruments are liable to the same

reversal; but on the South-Eastern line, where the system of train register books was introduced at the same time with them, I can find no record among 940 reports of accidents I have examined of any mistake occurring in working the Block System.

At the same time, in Preece's instrument this chance of error is guarded against.

Instead of my pointing out (as I have done in the Report to Government), how far I consider each system meets the demands of the rules I deduce, I think this meeting had better express an opinion on the essential requirements of an instrument, and then let the advocate of each system shew how many of those essential requirements his system meets.

All of the Single wire systems are generally used for "Non-following Block." But where they are used on Single Lines for "Non-meeting Block" the rule is that no other train goes into the section in either direction.

In India, however, to judge from the traffic I have seen, we get into a complication on Single Lines, which is not contemplated with either of these systems, viz:—

That the line being blocked a-head for trains coming up it, any number may go down with time intervals, that is, the Permissive Block System which has proved so dangerous in England, plus the extra chance of meeting a train.

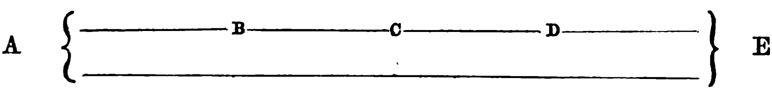
Now, if Absolute Block is to be introduced in India, either this plan must be prohibited, or we must provide for

A NON-FOLLOWING BLOCK

plus

A NON-MEETING BLOCK.

I propose to do this by two wires, each worked by Single Wire System Instruments thus—



The through wire working from Crossing-station A to Crossing-station E, is proposed for the Non-meeting Block. The wire running from A through cabins B C D to E is for the Non-following Block, and allows of $(n—1)$ trains travelling on it at the same time.

Upon this suggestion being made to the Telegraph Superintendent of the South-Western Railway, he immediately said

"Such a system, if combined with a Train Register, is perfect."

Being made to Colonel Yolland, he said

"No such system is perfect without a Train Staff."

I, however, give the suggestion here, as I believe it worth considering, and if there be time, discussing, more especially as I cannot but look on the "Train Staff and Ticket" system otherwise than only a variation of the condemned "Permissive Block."

Whilst studying the subject of the English Block Systems, I have learnt something of what is being done in the same direction in Germany; and, as this is so totally different to the English systems, it will be interesting to describe it. In German Railways the system has hitherto been one Talking Telegraph on the Morse system, from station to station, plus one set of bells.

These bells, which are at all the signal cabins, are rung from the station whenever a train is coming.

They have, however, this addition, viz:—that by the insertion of a particular key in a commutator attached to the iron pillar which carries the bell, a code signal (according to whichever key may be used,) is sent to and received on a separate Morse instrument at the station.

This system, however, not being considered sufficient, it was determined to introduce the block.

So, instead of leaving it to each company to try their own experiments, and perhaps try what has failed before only to fail again, a conference was held at Berlin, on the 1st December, 1870, at which were present either directors, chief engineers, or telegraph superintendents nominated by 17 of the principal German Railways, and also Baron von Weber, Consulting Engineer at the Royal and Imperial Ministry of Commerce; Inspector Tiremper, of the Imperial General Railway Inspection at Vienna; and Dr. Werner Siemens.

At this conference the following principles were fixed upon:—

1. For railways with a considerable traffic and comparatively few stations, it appears peremptorily necessary to establish intermediate stations.
2. In case one intermediate station should suffice between two railway stations, and the distance between the two stations be more than a German mile (4 miles English), the Morse system should be preferred. When the stages are shorter and the traffic greater, the block system is recommended.

3. The signals of the block system should be visible, not audible; and the signalman should be unable to remove (or change) on his own account the danger signal of *his* station.
4. The bell-signals, as at present in use along the entire length of the lines are not to be dispensed with, in order to allow of the block instruments being worked on its wires.
5. It appears undesirable to combine with the block system either a system of semaphores* worked by electricity, or that a signal by alarum† be added to the visible signal.
6. It appears unadvisable to make the block signal directly visible to the attendants of the train, until arrangements have been found which offer the same reliability for the mechanical giving of the signals, as the contemplated simple block system does; but the signal-man, after having received the electrical signal, must give the corresponding visible signal.
7. It is desirable that from every block-station a correspondence with the railway station be rendered possible by means of either stationary or portable telegraph-instruments, besides the block-signal arrangements.
8. For ballast-trains which return on the same track without reaching a station, particular provision ought to be made.

In consequence of the opinion of this conference, Messrs. Siemens Brothers, made an instrument which has been adopted by most of the German Railways, and of which the following is a brief description:—

The instrument which is generally calculated to work a distance of 20 miles, on wire of about No. 8 size, is a magneto-inductor, and therefore requires no battery. It consists of a cast iron box with two windows in front, four knobs on the top, and a crank handle on the side, besides a bell. Inside the windows are red and white discs which make the blocking apparatus.

The discs are worked by a series of alternate + *and* — currents generated by turning the crank handle. The bell is worked by either + *or* — currents only. The four knobs work the commutators, according to the direction in which it is required to send either + *and* — currents *alternately*, so as to work the discs or + *or* — currents *only* so as to work the bells. It will be easily understood that, as the discs require both

* This means the outside semaphores, not miniatures.

† i. e., Bell code signal.—This is directly opposed to the whole English system, of which the Bell code is an essential item.

currents to move them, not only can code signals be sent on the bells without disturbing the discs, but also these latter cannot be disturbed by lightning.

The instrument is made of two patterns, viz: for a double track or "Non-following block," and for a single track or "Non-meeting block."

Since the apparatus was first designed, to meet the demands of the conference, the following additions and improvements, called for by experience, and which can be used or not as required, have been made, viz:—It will interlock with the outside semaphore, either so that "line clear" cannot be given back on the instrument until the outside semaphore is set to "danger," or so that the outside semaphore can not be et to safety, until "line clear" has been received on the instrument. It can also be made to interlock with points.

It will be seen then that the instrument complies with the following rules that I have deduced from accidents, viz: 11—17—23—24—and obviates the objection of rule 10.

The great advantage of the instrument is, however, that it requires no batteries, and, therefore, when once purchased there is an end to the expense; besides which it requires no adjustment whatever.

If it be used then to carry out the system properly (instead of mixing it with the Morse, as on the German lines), there should be Permanent Magneto-Alphabetical instruments on all the local circuits, when any railway adopting two such instruments in combination would be completely independent of lightning reversing the block signals, and, in three offices out of every four of Batteries and Signallers.

The reason that these could not be dispensed with entirely is, that experiments already made in India have shewn that we cannot signal on Magneto-Inductor Alphabetical instruments at a greater distance than 80 miles of No. 1 wire (say 50 miles No. 4), and that even in fair weather; so that although all local circuits might well be worked with such instruments, the through wires connecting Engine-station to Engine-station, must be worked with some sort of Morse or Needle system.

Should the German instrument want repair (which it is absurd to suppose it never will) it will require more skill to repair than either of the English systems; but the advantages of getting rid of skilled signallers and of batteries seems so great that this latter question is not worthy of consideration.

If Alphabetical Instruments are not introduced with it then half its total advantage over the English systems (viz.: getting rid of batteries) is lost.

In conclusion it will be as well to compare with the rules which I have deduced from the English Railway Accidents, those laid down at the German Conference (which I had not seen when I wrote the former), and also the working of Siemen's instrument, when it will be found that, although only two rules actually correspond, the spirit of the latter not only does not disagree with mine, but that the instrument made to suit them has, from experience, been extended to include four of mine, to none of which it is opposed.

DISCUSSION.

Mr. Preece having given a description of the different instruments and models exhibited on the table—

Mr. Carr, at the invitation of the Chairman, described his system of working by means of a model exhibited. The chief object in his plan, he said, was to do away with signalling. In the case of all the instruments before them it was necessary to have two signalmen at work, and, to effect the transmission of the electrical signals, visual signals at intervals were necessary. It would be seen on the model that the signal was transmitted from the leading engine on to the following one. The signal was about the size of an ordinary steam gauge, and was visible night and day. It was an advantage that the power which constitutes the signal can be carried through points or crossings of the main line, and as long as those were open the signal "stop" was given to the following engine. Directly the points were closed the signal was given, "go on." [Mr. Carr illustrated the action of the apparatus.]

The Chairman inquired whether it was an electrical or mechanical arrangement, to which Mr. Carr replied entirely electrical.

Mr. Malcolm (of the Board of Trade), rising on the Chairman's invitation, said he had come to learn rather than to speak, and his views would not be worth hearing. He was anxious to hear the views of the various gentlemen present on this interesting subject, in order

that he might inform his own mind thereon. The matter was so entirely within the cognisance of electrical engineers, that he was afraid the views of one who had to inform himself upon it were not worth hearing. He might, however, be allowed to correct a slight mistake which Mr. Preece made as to the practice of the Board of Trade as to requiring the adoption of the block system on the opening of every new line. As a matter of fact, the Board of Trade had no power to require this system to be set up, and they did not require on the opening of a new line that the line should be split up into sections, and that electrical communication should be made between the various sections. He apprehended Mr. Preece rather meant that the block system went further than that, and carried with it such things as the interlocking of points and signals; and Mr. Preece was quite correct in saying the Board of Trade did require that the points and signals should in every case be interlocked: that was, that at every opening on to the main line the points and signals should be locked. With that exception, the explanations of Mr. Preece of the practice of the Board of Trade were perfectly correct.

Mr. W. H. Barlow said he was hardly prepared to speak this evening, but he had noted down a few remarks upon the paper. He would refer to one point of the paper which seemed to imply that the improved safety of railways was due to the inspection of the Board of Trade. Now, nobody more appreciated than he did the talents of the gentlemen at the Board of Trade. He believed they had greatly assisted in improving the safety of railways, but railway companies had another very urgent call upon them to improve the safety of railway travelling—that was the Act of Lord Campbell, which imposed heavy penalties in regard to persons killed and injured; and, therefore, though some credit was due to the Board of Trade, and some to Lord Campbell's Act, yet he thought some was due to energy and ability of the officers of the railways.

The point had been mentioned as to the number of accidents, compared with length of line, in the south of England and in the north, and the author expressed himself unable to account for the difference in the number of accidents in these cases. Probably he had not so intimate an acquaintance with the nature of the traffic. In the south the railways were comparatively short, and the traffic was nearly all passenger traffic, or mixed with a little goods: but when they came to such lines as the Midland and London and North Western, they

got a mixture of the most incongruous traffic. There was not only a great passenger traffic at high speed, but a heavy goods and mineral traffic, and sidings at every few miles. Any person looking at the nature of the traffic on the Midland and London and North Western would see what a difficult thing it was to start a train from one end to go through to the other end without collision. It had to pass enormous coal trains, and each of these trains had to be shunted by telegraph; and if anything occurred to interrupt the course of a train—bad weather or anything else—if anything occurred to interrupt the due order of the train, the difficulty of carrying the train through was very great.

He would in the next place refer to what had been said with reference to the cost of the block system. It was put down in the paper at £60 per mile. He was afraid it was a great deal more than that. of course this referred to a rough average calculated on certain miles of the railway. If they took a line in the metropolitan districts, he knew what the cost of the block system was there. They had block stations at every half mile, and those stations were worked day and night by three eight-hour shifts, and the cost of each station, with signals and lights, was £240 per annum, or equal to £480 per mile per annum was the cost of the men alone, without anything else for working that piece of line. Some of the men did other duties, and if the block system were done away with, or if the men were done away with, there would be the residuum of the expense left; so that they could not charge that all to the block system, and it was only when they came to two or three mile lengths that he thought it possible the sum stated by Mr. Preece represented the cost of the block system. It was stated in the paper that "experience showed that the block system increased the capabilities of a railway." He did not know what that experience was. He (Mr. Barlow) certainly thought the block system improved the safety of the railway, but he did not see how it increased its capabilities. He observed another remark which required attention. It was claimed for the block system that one of its advantages was, it led the driver to keep a good look-out. He thought there was a difficulty about this looking out. The present block system was made by one signalman sending to another signalman: this signalman worked a signal on the line, and the driver had to keep a look-out to see it. Now, it was known to all who were acquainted with railways, that occasionally

drivers, especially in thick weather, went by signals without seeing them, and, in the case of fogs, the Company was driven to all sorts of signals. Fog signals were put down within sight of the signals, and it was upon these explosions of gunpowder the safety of the train depended in cases of fog. It was with reference to that state of things that the contrivance shown by Mr. Carr had been designed; and this was intended to give the signal direct upon the engine, and the difficulty of passing signals in a fog was overcome. Another important thing was that the action of the apparatus being automatic, the large expense of signalmen was avoided, to which he just now referred. There was one other remark which tended in the same direction. The author said :—"Where we are dependent upon human agencies we are very liable to error of work,"—and that, he thought, was another reason why they should endeavour to establish the automatic system, instead of working by human agencies. He was glad to see the attention of this large meeting directed to so important a subject.

Mr. G. Leeman, M.P., said, like his friend who spoke first, he had come rather to listen, and to gather information, than to impart it to others. Practically, he knew very little about the working of the various electrical apparatus before them, but there were one or two remarks which fell from the gentleman who read the first paper, on which he would say a few words. One remark of that gentleman was, that he feared, as far as the directors of railways were concerned, there was no disposition to adopt this Block system, because of the expense in which it would involve the companies. He was bound to say, on behalf of the railway companies, and especially of the one which he represented, and of which he was the vice-chairman—a railway of 1,400 miles—that was a remark which he did not think applied to railway companies generally. He did not think, perhaps the author intended what his remark implied. He (Mr. Leeman) would say, on behalf of that particular company to which he referred, that there was no amount of expenditure, necessary to secure the public safety, which that body of directors was not prepared to incur; and he did not think the manager, nor did he believe the managers of other companies, would endorse what was implied in the remark made, viz:—that the managers made suggestions to their directors which the directors were not prepared to adopt. He did not believe that was the fact, certainly not with regard to his own company; and

he thought the course recently pursued by the large companies now adopting this system as fast as they could—The London and North Western, the Great Northern, the Midland and others—was a sufficient answer to that remark. He thought the experience of the last 30 or 40 years in this country had shown that they might leave to railway directors themselves the question of the adoption or otherwise of what was desirable for the public safety, and for public working; and he thought the men whose portraits were round that room, the course of action pursued by most of those men, was in itself an answer to the remarks, so far as it was intended to imply that either managers of railways, or engineers of railways had not been met by the hands of direction in the manner they ought to have been. He was addressing a body of gentlemen mainly associated with the system of telegraphy. There was no body for whom he had greater personal respect, nor for whom the railway directors of the country had greater respect, and he was sure that was proved by the extent to which telegraphs had been adopted everywhere. Where had they a railway where there was not a telegraph to work it with? At this moment when the telegraphs of the country had passed into the hands of the Government, and when they had these large claims against the Government in respect of the telegraphs, it formed the best answer that could be given to the remark to which he referred, that the railway directors had retained lines of wires on their railways for the purpose of working them by the aid of the telegraph. They adopted that course before the transference of the telegraphs took place for the safe working of their own traffic; but the object of Mr. Preece's paper seemed to be to enforce upon the companies the adoption of this Block system. But he thought that paper must have been prepared before the evidence which was given within the last month or six weeks before the Committee which had sat in the House of Lords, which had heard all the evidence of all the practical men of this country on the subject of enforcing the Block system upon railways. What was that evidence? There were gentlemen who came from one or two of the railway companies, one of whom, from the South-Western, seemed to adopt that view; but what was the evidence of the other practical men—the large railway managers? It was that, while they believed the Block system might do a great deal for the promotion of public safety, yet they could not find a single man scarcely who did not state that if an accident happened

with the Block system, far greater evil consequences would arise under that system than under the present one. What was the course pursued by the Committee? They reported it was not desirable to enforce upon railway companies this Block system, till they had had some further experience of the working of it. He did not say a word with regard to the different models put before them. There were rules, running up to No. 24, to be adopted if this Block system was rightly applied. Was not the fact of the necessity of 24 rules being instilled into the men's minds before they could ensure the safety which the Block system was intended to ensure, the best possible answer to the paragraph which stated that 28 per cent. of the accidents on railways were due to human agency. He was afraid Mr. Preece knew but little of the practical working of railways. He (Mr. Leeman) had worked one for 25 years, and he would tell that gentleman that until he could ensure perfect infallibility he would have accidents under the Block system as well as under any other; and therefore, with the greatest respect to the gentlemen who worked the telegraphs, he hoped the Telegraph Engineers of this country would not allow themselves to be run away with by the arguments of the gentlemen who had addressed them, and to suppose that, with all their good intentions they could prevent accidents on Railways under the Block system, which did not possess the principle of infallibility, and endeavour to enforce the adoption of that system, whether people would or not. With regard to the line with which he was connected, he endorsed every word which fell from his friend Mr. Barlow. Would any man tell him that under any Block system that could be adopted perfect immunity from accident could be ensured? The Block system was employed on a portion of his line, and he could tell them that the accidents which occurred on the North Western Railway no Block system in the world could have been the means of preventing. He had said now what he felt himself called upon to say in the House of Commons the year before last, when Sir Selwyn Ibbetson brought before the House the number of accidents which occurred upon railways, and he then took the opportunity of showing how utterly insignificant was the number of accidents on railways compared with the millions of miles travelled by the millions of people using the railways, and the hundreds of millions of tons of goods carried on them; and when they took into account the various sources from which this traffic arose, it was a perfect marvel that there

were not more accidents than there had been upon our railways. He was not one who thought it right to enforce upon railway companies the Block system, and especially upon those parts of the railway which consist of short branch lines. The object was to enforce the Block system upon the whole of the lines. Adopt it if they pleased on the main lines—such as between London and Edinburgh, London and Liverpool, and London and Bristol. They had heard about the interlocking, and the system of Saxby. They knew the parties who supplied these apparatus had been pressing them for months and months, but they could not get what they wanted upon the main lines; but with respect to the little single line branches let them continue to be worked by the staff, which was a better Block system than any electrical Block system that could be introduced. To talk about being bound and forced to adopt this Block system would be an injustice to railway companies, however ready they might be to put their hands into their pockets.

Captain Mallock feared he was misunderstood about the Block system for single lines; but the remarks he read upon that were those of Mr. Hawkshaw. With regard to the number of accidents, and the Block system being required to prevent accident, the reports of the Board of Trade showed that the adoption of that system would probably have been the means of preventing many of the accidents which appeared in the lists of those reports, and the circumstances attending those accidents he submitted bore out the propriety of the rules, or more properly the axioms he had brought forward in preparing such a system. Certainly this proved that accidents had occurred from the want of the Block system.

Mr. Leeman: It proves that accidents have occurred; but that they have occurred from the want of the Block system I utterly deny.

The discussion was then adjourned till the next Meeting.

The following Candidates were ballotted for and duly elected:—

Frederick Hamilton . . .	Greenwich.
Henry Mance	Kurrachee.
Thomas Rowe	Manchester.

The Seventeenth Ordinary General Meeting was held on May 14th, 1873, Mr. C. W. Siemens (Past-President), in the Chair.

Renewed discussion on Mr. W. H. Preece and Captain Mallock's papers "On the Block System of Working Railways."

The Chairman said a letter had been received from Mr. Walker, with reference to the discussion at the last meeting. Mr. Walker was prevented being present to explain his views verbally, but he had sent those views in writing; and though it was not usual, the Council thought it would be agreeable to the Society if this letter were read by the Secretary:—

"South Eastern Railway,
"Electric Telegraph,
"Tonbridge, *May 13th,* 1873.

"Dear Sir,

"Captain Mallock, in his paper on the Block System, read before the Society on the 23rd ultimo, expresses opinions on my telegraph semaphores which cannot fail to convey false impressions to the prejudice of the instruments. He thinks—

"1. The springs of the commutators, worked by the plungers, weak in principle.

"2. Liable to have reverse signals given on them (the semaphores) by atmospheric electricity.

"3. The plungers so much alike that a man in a hurry may make a mistake.

"The facts in each case are otherwise :—

"1. In the London District where there are 180 commutators and semaphores, and the traffic is very great, and the wear and tear excessive, not a dozen springs per annum are broken. And no repair is so

simple as that of a spring. Draw a single screw, lift off the old spring and screw down the new one. At a station a mile out of London, where there are six commutators, at which 976 trains with 10,305 distinct signals (and 16,707 figures entered in the register) were signalled last Easter Monday, and daily in proportion, only two springs were replaced in eighteen months.

"2. We have no instance on record of lightning reversing a signal, nor is it surprising; the instrument being less liable than, perhaps, any other to be effectively acted on by lightning.

"In passing, I may add that complete signals being a combination of bell-strokes and the moving of semaphore arms, it would be next to impossible for lightning, even if effective, to make such a signal as would mislead the signalmen.

"3. With regard to the plungers being so much alike. This hardly requires explanation. But to those who have not seen them it is enough to say that they are five and a half ($5\frac{1}{2}$) inches apart; that they are one above the other; that the *upper* one is used to put the arms *up*, and the *lower* to put them down; the upper is a *white* knob, and the lower a *black* knob. Besides which the signal made is visible to the man who makes it.

"These semaphores, of which on parts of this railway alone 202 were in use in June last, have been worked so efficiently that I am at this moment carrying out instructions to substitute bell-semaphores for bells on the whole of the Railway and its several branches, which will absorb in all 418 instruments.

"It is hardly correct to call mine a single-wire 'system,' which reads as if one wire was essential.

"The experience of $21\frac{1}{2}$ years with signal bells, and eight years with bell-semaphores has proved that a single wire is efficient and adequate; but there is nothing in the construction of the apparatus to prevent two wires or three being adopted by those who prefer it.

"On the other hand, I cannot conclude without mentioning with thanks that Captain Mallock, in his communication, has shown more distinctly than I remember to have seen it elsewhere, to whom the Railway Companies are indebted for the introduction into the Block System of the Single Stroke Bell and Bell Codes, deprived of which the Semaphores and Index Instruments—mine equally with the rest—and some of the needle systems, would be impracticable.

"Possibly it may not be out of order to read this letter at the meeting appointed for to-morrow.

"I am, dear Sir,

"Yours truly,

"CHARLES V. WALKER.

"GEORGE PREECE, Esq.,

"Sec. Soc. of Telegraph Engineers."

The CHAIRMAN said he would now call upon the meeting to continue the discussion, which was commenced at the last meeting. He saw before him a great many instruments which had not yet been explained to the meeting, and which no doubt would form a very important element in the discussion. He would, therefore, call upon gentlemen by whom these instruments were exhibited to explain them to the meeting before they proceeded further with the discussion. Amongst the various signalling apparatus before him he saw one from the establishment with which he was connected, and he would ask Mr. Risch to explain it.

Mr. RISCH then proceeded to explain—

"SIEMENS' MAGNETO-ELECTRIC BLOCK-INSTRUMENTS, INTERLOCKING WITH SEMAPHORES, POINTS, &c,"

as follows. He said:—

The instruments before you are constructed on the "non-following" principle, *i. e.*, they are made to block the line behind a train, so that no two trains are allowed at the same time between two signalling stations. Their chief peculiarity consists in the disuse of voltaic batteries for working the electrical signals, and, in the latter, being made to interlock with the outside visual signals (semaphores, &c.) in such a manner that a signalman is not able to take his outside signal "off" unless the station in advance of him has electrically given the signal, "line clear;" for the electrical signal is, by mechanical means, made to interlock firmly with the lever actuating the outside semaphore, arms, lamps, &c., so that the man cannot move it at all when "train on line" shows on the corresponding aperture of the electrical instrument, and only the action of the electrical signal "line clear," coming in can disengage that lever from the locking mechanism, and set it free to be moved, upon which the signal may be taken off. Nor can the man in advance return the electrical signal, "line clear," unless he has first "*put on*" *his own signal.* He cannot,

therefore, cause any danger to an advancing train, either by neglecting to put on his own visual block (for in that case the signal farther from him behind the train would *remain* blocked and locked) nor can he in the rear cause danger by taking his signal off before electrical permission for doing so has been given him (for he simply cannot move his lever, it being locked when blocked), but having, with permission, taken his signal off, he is positive that the signal behind him is protecting the train.

The battery is replaced by the well-known Siemens' magneto-inductor, constructed so as to give both reversals and single currents as required. Bell signals are given by single currents in the direction of the moving train, and the block signals by a series of reversals in the opposite direction.

I beg you to understand that the models on the table, being made in Germany, show "line clear" by the semaphore arms pointing upwards, contrary to the usage of this country, where they are made to point downwards when indicating the same. Their adaption for English use is, of course, easy, and is shown on the diagram above.

The advantages of this system may be summed up thus:—

1. By the disuse of galvanic batteries, failures, arising from their well-known inherent defects, are eliminated.

2. Accidental currents, such as intermittent contacts, atmospheric electricity, &c., cannot produce or remove an electrical block signal, twelve or more *reversals* being required to produce one signal.

3. A check has been put upon the working of the "human" machinery attendant upon the action of both the electrical and visual signals, so that by neglect of duty the safety of a train in motion cannot be endangered.

4. A system of these instruments connected by one line wire only can be made to work reliably. Their adaption to any of the known three wire systems is an easy matter.

5. A combination of these instruments interlocking with the points or crossings, &c., and the line signals of a crowded station, or where much shunting is going on, can be made to act in such a manner that any line signal cannot be taken off unless all points or crossings of such respective line, stand correctly to the direction of the advancing train.

The details of construction are shown on the diagram. There are two apertures in each instrument, the one to the right, say, for the "up-line," the one to the left for the "down-line." Behind each aperture is a flap in form of a segment of a circle which turns upon a horizontal axis, and is colored partly red, partly white. Its upward or downward motion thereby brings either the *red* or the *white* field behind the corresponding apertures. The circumferences of these segments are provided with teeth, into which play anchor-escapements in connection with polarized tongues oscillating between the poles of electro-magnets. There are, therefore, two such segments, two such tongues, and two electro-magnets, one for the "up" and one for the "down" aperture in each instrument.

Above each aperture is a plunger knob which acts upon a commutating lever, similar to a Morse key, and, when at rest, is in communication with the receiving contact, and when depressed, with the sending contact. The electro-magnets of each aperture are thus, when at rest, in constant communication with the line wire in the direction whence a train leaves a signal box and earth, and when depressed, in the direction whence a train arrives at a signal box and passes over the acting inductor to earth. The magneto-currents are produced by turning the small crank handle at the right of the instrument, and it is evident from the above, and I deem it a further improvement with these instruments, that any uninitiated person, by playfully turning the handle of the inductor or touching a plunger knob, cannot wantonly produce a dangerous signal; for, by the first action, the inductor not being in contact with either line, produces no currents, and by the second, although the sending contact is made, no currents can flow into either line while the inductor is not in action. Therefore, only a person properly instructed, by combining both actions, can effect a signal on the corresponding aperture.

The segment when showing red (train on line) is in its upward position, which is the natural position of rest, ready for receiving, *i. e.*, ready for the signal to be taken off, so that white (line clear) may appear. In this position the segment has a tendency to drop, but is prevented by the anchor escapement. Arriving currents, therefore, by the oscillation of the escapement, only permit the descent of the segment. When sending, *i. e.*, when blocking one-self, a counterpoise is made to rest on a tail-piece projecting from the segment beyond its centre, by the depression of the sending plunger, giving the segment

a bias in the upward direction, and the only work the currents produced have to perform, is to regulate the bias of the ascending segment. As stated before, twelve positive and twelve negative currents are required to complete one signal. A succession of accidental currents of equal direction could, at their worst, produce the 24th part of a signal only.

Behind each sending plunger there is another smaller plunger, by the depression of which, and the coetaneous turning of the transmitted handle, the signalling bells are sounded to announce the advance of a train or its nature—the inductor being so constructed that, by the depression of a small plunger, currents in one direction only enter the line, while by the depression of the former reversals are transmitted. It will thereby be understood how the signalman works the single current bells either way, in the same wire with the double current block signals without affecting the latter.

For railways, however, where a single line wire does not afford sufficient protection, the instruments are easily connected so as to suit any of the known three-wire systems; and, with the same facility, the commutating parts may be so arranged that a signalman may not be able to act upon his own electrical signal, so as to be under the entire control of his right and left hand neighbour. The mechanism which works the semaphore lever is applied directly below each aperture, and the levers working the corresponding semaphores are fixed to the right and left of the case of the instrument, so that it appears at a glance in respect to which lever a signal is being given. A sliding rod is fixed perpendicular with the sending plunger, and just below the commutating lever. It carries a squared-edged shoulder, or nose, and has a tendency given to it to fly upwards by the tension of a spring-pawl on which it rests. This spring-pawl slides upon the circumference of a disc, with one incision, which is fixed to the axle of a semaphore lever. Lateral to the sliding rod a crank lever is fixed, one arm of which is made to bear against the square-edged shoulder, or nose, in such a manner that, when the spring-pawl rides upon the circumference of the disc, it (the shoulder) has pushed the crank one side, and through an incision in the axle of the segment. In this position the sending plunger is locked. It cannot be depressed—the spring-pawl resting on the circumference having raised the sliding rod, holds the commutating lever firmly against the receiving contact, and makes the plunger perfectly rigid.

But the semaphore arm in this position is *not* locked for it shows "Line clear," and is at liberty any time to be turned to danger when the corresponding aperture of the electrical signal shows white. Will the man take the signal off the station in his rear, he is forced to turn his own signal on first, for only by that means the incision on the circumference of the disc fixed to the axle of the semaphore lever is placed below the spring-pawl. Now, the sending plunger may be depressed, the spring-pawl descend into the incision below it and the commutating lever be brought to bear against the sending contact. The tension of a spring fixed to the short arm of the lateral crank-lever, thereby causes it to return to its original position through the incision on the axle of the segment, and, while a right-angular set-off on the edge of the upper arm of the crank-lever has mounted over the square edged shoulder or nose of the depressed sliding rod, the partial rotation of the segment has displaced the incision on its axis and locked the crank-lever, which thus retains the spring-pawl in its cavity on the disc, fixed to the axle of the semaphore lever, and holds it locked in the blocked position as long as the electrical position of the signal shows danger. Only on this signal being removed by the station in advance, *i. e.*, the partial rotation of the segment, the incision on its axis returns to its former position, and by its own elasticity the spring-pawl can raise itself clear of the incision, carrying with it the sliding rod which again throws the crank-lever aside, and thereupon the semaphore-lever is free to be turned to "line clear."

For the better protection of crowded stations and stations where much shunting is carried on, a combination of these instruments, interlocking one with the other and with their corresponding points or crossings, is arranged so that a signal cannot be taken off any semaphore intended to permit trains to enter or pass through, unless all points or crossings of such respective line stand locked into the correct position for the direction of such coming train.

In all the various adaptations and combinations of these instruments, their main features remain—(1) the abandonment of batteries—(2) the employment of a series of inversed currents for producing the signals, and—(3) the interlocking of the electrical apparatus with the levers working the outside signals, so as to prevent mischief being done by careless attendants.

Mr. Carr then explained Carr and Barlow's Electric Automatic

Block System of Signalling for Railways. The object effected by our invention is that no train passing along a line of railway can approach within a limited distance of the preceding train without receiving a warning signal. The distance at which trains are to be kept apart may be any suitable distance, and the apparatus works automatically.

To obtain this result we employ a system of apparatus arranged as follows:—Along the line are places, at which a signal can be trans-transmitted electrically on to the engine, or on to any carriage of a train, through insulated metal bars. These places occupy similar positions to those of the home signals of the present system.

The insulated metal surface is not permanently in electric connection with the insulated wire from the apparatus hereinafter described, but is connected at the time that a train moves past it. This may be effected in various ways. The insulated metal surface may be a roller supported by a spring, and having a contact maker below it enclosed in a suitable box, coupled-up with the insulated wire from the apparatus. In this case a curved metal bar carried by the engine (or it might be other convenient part of a train) may act upon the roller and depress it so as to put the roller, and bar on the engine in electric connection with the insulated wire, or the insulated metal surface may be a bar fixed parallel with the rail, and a roller or spring be mounted on the engine so that it shall come into contact with it. In this case, in order to put the insulated bar in electric connection with the insulated wire at the time when a train is moving past it, we prefer to cause the wheels of the train to act upon and depress another metal bar placed close alongside of one of the rails. This bar is mounted on a number of short links, and is held up by a weight, or springs. The axis of one of the links forms part of a contact maker enclosed in a suitable box. The contact maker is constructed so that when the bar is held up the insulated wire and insulated bar are not in electric connection with one another, but when the bar is depressed by the wheels of a train passing over it, the contact maker completes the connection between the wire and insulated bar. The spring or roller which is mounted on the engine and which comes into contact with the insulated bar is coupled by an insulated wire to one end of the coil of an electro-magnet forming part of the signal instrument upon the engine. The opposite end of the coil may either be coupled to any suitable part of the engine, so that the connection

to earth may be completed through the lines of rail, or the return current may be conveyed back by a second insulated wire connected like the other wire to a contact maker, to be moved simultaneously with the other contact maker and by similar means. The instrument on the engine is made with a moveable signal disc, arm or other contrivance, which remains in its normal position until an electric current passing through the electro-magnet above-mentioned, moves it. When the signal disc or arm is in its normal position, it represents the signal "stop," and when it has been moved to the other it represents "go on." If a current passes through the instrument on the engine the signal is moved to "go on"; if no current passes through, the signal remains in its normal position and represents the signal "stop." The "stop" signal carried by the disc or arm is a red glass, and the "go on" signal a white glass. When the signal disc or arm is in its normal position the red glass will be seen through an aperture in the front of the casing of the instrument. If the signal disc or arm is moved to "go on" the red glass is moved away and the white glass comes behind the aperture. The instrument is made so that a lamp may be placed behind the signal disc or arm, in order that at night a red or a white signal light may be shewn.

In front of the signal disc or arm is a screen, which, as the engine passes the insulated metal surface hereinbefore mentioned, is moved away mechanically or electrically by a trigger or spring fixed on the engine, and so displays the signal, and at the same time and in the same manner a bell is rung to attract the attention of the driver to the signal exhibited. As soon as the driver has seen the signal, he by depressing a finger key, replaces the screen in front of it, and in so doing puts back the signal disc or arm (if it has been moved). This putting back the screen may be done either by the engine driver, or by an automatic arrangement, but we prefer that the engine driver should, as a part of his duty, be required to put the screen back into its normal position.

By the means above described, the signal "stop" or "go on" is displayed to the engine driver of the approaching train.

The automatic arrangement is as follows: in between the places where signals are transmitted on to the trains (which for convenience we will call stations) are other places at which the trains, passing over a lever or other mechanical or electrical apparatus, cause by means of

an instrument (which we call the "Transmitter"), the insulated bar at the station the train has just left to be uncoupled from a battery, and the insulated bar at the station immediately behind the one that the train has just left to be coupled up to its battery. For example, let A, B, C, D, &c., be the stations where (when the train arrives) the signals are received on the engine, and let *a*, *b*, *c*, *d*, &c., be the other places between A B, B C, C D, and D E, &c., where the levers are situated; from *a* to A is a wire with a battery, the extremity at A is connected, as before described, to the insulated metal surface at A; the other extremity at *a* passes into the "Transmitter," where the coupling and uncoupling is effected. When the extremity *a* in the "Transmitter" is coupled to the battery, the signal "go on" will be given at A, and will be exhibited on the engine when it arrives there. When the extremity at *a* is uncoupled, the signal "stop" will be given at A.

The uncoupling at *a* is effected by the passage of a train over the lever at *a*, and the coupling by an electro-magnetic apparatus brought into action by the passage of a train over the lever at *b*, a wire with a battery being laid between *a* and *b*. The coupling and uncoupling at *b*, *c*, *d*, &c., are similarly arranged.

Now let us suppose a train to be starting from A, it arrives at the ever at *a* passing over it uncouples the circuit *a* A, thus leaving the signal "stop" at A, the train going on will arrive at B and receiving the signal "go on" will proceed, passing over the lever *b*, in doing so it uncouples the current *b* B, thus leaving the signal "stop" at B and at the same time by the wire *b a* brings in action the electro-magnetic apparatus at *a* which couples up the circuit *a* A, thus leaving signal "go on" at A, and so on; thus, without the aid of signalmen, the line in rear of a train is automatically blocked for any pre-determined distance; but as exceptional circumstances might arise which would make it necessary for a man to have the control, we have connected to the wires from lever to lever (viz., *a b*, *b c*, and so on) a "Control instrument," which indicates the signal received on the engine, and by means of which a signal can be given at any of the stations A, B, C, &c., if required, but is so arranged that signal "go on" cannot be given unless the line is clear ahead.

For repeating the signal received in the instrument on the engine, back to the control instrument, we have in the instrument on the engine (described above) an arrangement, that when the disc or arm is

moved, a contact is, by the movement of such disc or arm, caused to be made between the extremities of two wires, one coupled to "earth" and the other put into electric connection with a spring or roller, which comes into contact with an insulated bar similar to that already described, and situated at a distance of about twenty yards or thereabouts from it, and nearer to the station to which the train is approaching. The wire from the insulated bar above-mentioned is connected to the control instrument, which is a similar instrument to that on the engine so that if the signal disc in the instrument on the engine is moved, the disc in the instrument will be moved also, and *vice versa*. Thus the man at the station knows what signal the engine driver has received.

In connection with this instrument is a time register, which is arranged so as to register the time of the passage of each train over the levers and bars, the signals given and received by the control instrument, and the signals received by the engine.

The registering apparatus consists of a sheet of paper wound upon a cylinder connected with a clock in the signal station, this sheet is divided into spaces to indicate the hours and minutes, a marker is brought down on to the paper by means of an electro-magnet each time that the various signals are given and received.

The arrangement above described is suitable for lines of railway on which every train stops at each of the stations, but for lines of railway where this is not the case we modify the arrangement by employing, in addition to the insulated bar situated at each station itself, another corresponding bar at a distance in rear of the station to act as a distance signal apparatus, so that a train receiving a signal of "stop" at the distance signal may slacken its speed as it comes up to the station, when it will receive another signal, and either go on or stop according to the signal it receives.

Mr. Spagnoletti (responding to the call of the Chairman) said, it was due to the authors of the papers to thank them for bringing this subject before the Society; more especially for the pains they had taken in collecting the statistics and facts that they had arrived at. At the same time there were one or two things he could not quite hold with. He had had considerable experience in the working of railway telegraph signals, and he found that practical working was one thing, and theoretical working was another. There was great difficulty in bringing theory thoroughly into practice, and his expe-

rience told him that though it was most desirable, no doubt, to have a uniform system of working signalling apparatus, it was nevertheless a very difficult thing to effect, because there were so many circumstances in connection with the varied traffic on different portions of the line, that it was impossible to make one system alone applicable to the whole of the establishment. They had, for instance, one part of a line straight, and tolerably free from gradients and tunnels—and on those portions they found a difficulty in this way—though free from these things, there was the difficulty that there were express trains, stopping trains, goods, minerals, and fish trains, all running at full speeds. That showed the necessity of having the block stations fixed at such distances, according to the traffic, that the trains would follow each other, having regard to the rate of speed of the slowest train. If the distance of the blocking station was one mile on one side, and three miles on the other, they would see the difficulty there was to get the traffic to work regularly together, and where the stations were varied in the distance apart it was difficult to get the traffic to work evenly together along that portion of the line; and where gradients and tunnels occurred they required special regulations.

There were some portions of the line where they had done all they could to bring the block system to bear, but there were difficulties in doing so. They could not assimilate a line throughout the whole distance, and they found no one system was entirely applicable to a line generally. He would say further as to the necessity of making the stations equal distances apart, to suit the requirements of the traffic on crowded lines, where trains fast and slow ran intermixed; although this was desirable and necessary on such portions of the line as he had referred to, "it would of course not be possible to do so on those portions of the line where gradients were steep and long, and it would be apparent to all an absolute folly to put a station and signal for stopping trains on a descending gradient where trains could not safely and surely be pulled up, or if ascending in the opposite direction they were stopped and could not, from the rising gradient start again; very frequently on such portions of the line as well as at junction crossings, and draw-bridges it was necessary for safety to ask if the line was clear before a train starts, and through such varied requirements the absolute necessity of having an experienced telegraphist. who also thoroughly understands the working of a Railway so that the fullest advantages of the Telegraph could be applied and fitted to

the various requirements of the traffic was self evident, as in such portions of the line where the regular and general system was not considered sufficient for regulating the traffic, the Telegraph is resorted to, and the instructions for working such portions of the line were generally drawn out by the Telegraph Engineer.

The instrument before them which he should have the pleasure of explaining, was one which he had devoted a great deal of time and trouble to, in order to arrive at what was to his mind almost as perfect an apparatus as could be applied to the working of the block system. The instrument on the table had been at work on the Metropolitan Line 11 years. It had never failed in its working, and no accident had occurred through telegraph defects since the line had been opened. The same form of instrument was adopted by the Great Western Company, and was used on many of the branches including the Hammersmith and City Junction branch. The apparatus was extremely simple, and he had tried to assimilate it as much as possible to the signals on the line. The advantages claimed for the three wire system over the one wire system were as follows:— A one wire system requires that the instrument should be so made that the signals sent to the station in the rear should be held up by mechanical or magnetic appliances; because it was clear that on one wire, if an up and a down train have to be signalled on the same section at the same time, that unless the wire is left free after a signal is sent, the other could not be transmitted; therefore these instruments are so made that the signal given is held up by mechanical or magnetic means; hence any foreign agent, such as a flash of lightning, an earth current, a contact of the line wires may reverse the signal. When trains are signalled forward on the single wire system, the bell is only rung to the station in advance; no visual signal is given, and the station in advance then places the needle or indicator over to "train on line," or "danger" at the station in the rear, and that needle or indicator is so left, being mechanically or magnetically held in its position. The needle or indicator indicating the last signal sent, is only brought into use when sending the signal, and the line wire is always left with the two receiving indicators in circuit; hence the very indicators most important to protect are the two left most exposed, and liable to alteration by foreign agencies. If wires get in contact, the currents passing on the wires of long circuits naturally fly to the block wires. As block

sections are short and earth's are close at hand, therefore, in cases of contacts, the wires on the block instruments really attract an element of danger when contacts exist. Earth currents vary in their denomination, and thus the needles on the receiving instruments, being in direct communication with the earth, are changed and moved, as the earth current changes, and the signals are thus altered on the instrument as if they had been properly done so. There are many stations on a line of railway which are not required to be kept open all night, and are switched out with a one-wire system. Although there is no difficulty in switching out at night, he considered there is very great risk in switching the instruments *in* circuit, again in the morning, for the needles, or indicators, at the station switched out show all right when switched out, and they are mechanically or magnetically held in that position; and so, when the instrument is switched in again, they still remain so, although the stations on either side may have signalled a train to each other, perhaps, in both directions; and, under these circumstances, the centre station switching in of the three shows "line clear" in both directions when trains may be on both lines on either side, it is customary when switching a station in to give a bell signal to the next station, but, if a train has been signalled and the needle indicators show "train on line" at each side; the battery current of the centre station switching in being of a different nature to that which put the signals to "danger," will put the signal to "line clear," because when an instrument is switched, and it is not done till "line clear" is sent and received to and from the station on either side; and, therefore, when the signal is sent when the instrument is switched in in the morning, put the signal on the instrument in the same position as that from which it is sent, so that any signal sent by the stations when the intermediate station is switched out, its being switched in again and giving the signal, the line may be cleared by the sender of the signal, although he knows nothing of what trains are in the lengths. The alternative for this is to appoint night signalmen at such stations, and this means a great expense if many are required on a line, and specially adds to the cost in maintenance of the one-wire system.

Now, with the three-wire system with its undemagnetisable needle, none of these various effects can occur, for this reason—the signal is kept up by the current from the station the signal is sent from. If lightning comes, and it is of the same denomination as the current

being sent, no effect is seen; if it is of the opposite denomination, being of an instantaneous nature, the signal being kept up by the current, it only just gives a slight movement, and the disc has not time to fall, the current recovering it as instantaneously as the lightning leaves it. In cases of contact the effect is the same. In switching out and in, the permanent current being on, directly the Instruments are switched in circuit the currents on from either side shew the state of the line, and a person knows at once whether any train is on the lengths, and to which stations, and in which direction they are running, As he cuts the circuit in half, all he has to do is, that whatever signal he sees on his own Instruments, he sends on either to the station in the rear or advance the state of the line, no matter when he switches in, is evident and clear.

He begged to add that his Instruments are most simple in construction, having little or nothing in them to get out of order, simple to work, and clear and simple in arrangement. The only instructions a man wants is, that whatever signal you wish to give, press down the key, coloured and labelled accordingly. Only one signal at a time can be seen, and that, the one intended to be shewn. The exhaustion of the battery is registered by the Instrument itself, and through this arrangement an Instrument need hardly ever fail.

The instruments are worked thus:

Take A and B as two stations on double lines:

A has possession of section to B, and has the instrument at his station pegged down to "line clear." B has possession of the line to A, and has the instrument at his station pegged to "line clear." Before either station can obstruct the line they must get the station from which a train will approach to unpeg the white key "line clear," for them to peg down the red key "danger," hence the men at both ends know when the line is obstructed. When a train leaves A, he rings the bell 2 or 4 beats, to describe the passing of a passenger, goods, or mineral train, unpegs the white key, and presses down the red key "train on line" B pegs down the red key and keeps it pegged down till the train arrives, when he gives 3 beats on the bell "line clear," and unpegs the red key. A returns the 3 beats on the bell, and pegs down the white key. All is then ready for another signal.

When working single lines, junctions, gradients, and drawbridges, I prefer asking every time if line is clear before starting the train.

The instructions are short and simple, and I think the more they are so, the less liability we incur to confuse and complicate the working of the system.

The Metropolitan and District Railways have been worked by his instruments, and he had never had a single hitch in working the traffic in that time by the system adopted on those lines. The traffic is enormous as the following returns will shew:—

GENERAL STATISTICS REFERRING TO THE METROPOLITAN, METROPOLITAN DISTRICT, ST. JOHN'S WOOD AND HAMMERSMITH AND CITY RAILWAYS.

Number of Passengers each Year.

Year.	1st Class.	2nd Class.	3rd Class.	TOTAL.
1863	1,223,588	2,512,872	5,718,715	9,455,175
1864	1,256,894	2,490,912	7,974,083	11,721,889
1865	1,563,105	3,109,854	11,090,948	15,762,907
1866	2,198,130	4,262,131	15,298,553	21,758,814
1867	2,499,840	4,909,629	17,568,971	24,978,440
1868	2,977,063	6,050,502	20,732,487	29,760,052
1869	4,016,829	8,085,142	28,170,948	40,272,919
1870	4,578,885	9,374,074	30,409,833	44,362,792
1871	5,525,514	11,307,962	31,323,089	48,056,565
1872	5,426,961	11,140,692	30,149,135	46,716,788
Total	31,266,809	63,143,770	198,436,762	292,847,341

Number of Passengers in the greatest Week and greatest Day in each Year.

Year.	Whit Week.	Whit Monday.
1863	251,270	51,496
1864	268,210	59,724
1865	384,124	83,440
1866	484,900	107,697
1867	570,720	119,570
1868	650,746	129,050
1869	907,657	189,449
1870	1,043,986	216,573
1871	1,122,814	240,485
1872	1,091,832	332,139

Number of Trains each Year.

Year.	Passenger.	Goods.	Total.
1863	55,430	—	55,430
1864	77,004	—	77,004
1865	104,314	—	104,314
1866	147,458	2,503	149,961
1867	134,477	4,418	138,895
1868	182,913	5,737	188,650
1869	306,217	15,278	321,495
1870	335,317	21,272	356,589
1871	320,928	162,525	483,453
1872	353,220	30,586	383,806
	2,017,278	242,310	2,259,597

Number of Trains in one day (week day) ... 1,084

and when they saw such vast nnmbers carried and safely so, he thought it was a strong recommendation for the system by which such traffic was conducted so successfully.

Mr. Rudall said he had had some years experience in the signalling of trains, and was asked to devise a system for the Midland trains, which were very numerous. The line was composed of four lines from Leeds to Worcester, with trains in all directions, crossing each other. He thought it was desirable to adopt the simplest plan which was at that time followed, and he fell back on the single needle instrument, arranged with bells, as introduced by Mr. Clark. Captain Mallock referred to the London, Chatham and Dover line, and remarked upon the possibility of lightning affecting the needle, and the necessity of employing the demagnetizing needle of Mr. Varley. He was not himself a patentee, but his object was to employ the simplest instrument. Mr. Rudall proceeded to explain the working of the model exhibited by him. By means of the handle they acted upon a bell and needle according to the number of beats. On some portions of the line it was necessary to distinguish the trains to such an extent that they had adopted two letters for two trains, to distinguish goods and passenger trains.

If they required to block the line the signal was made and acknowledged by the man at the other end, who pegged his needle over. This was all done upon one instrument, which in practice

was a great advantage. At some stations they had ten instruments going, and trains following at intervals of 3, 5, and 10 minutes. These trains had to cross the different lines, and this was done satisfactorily by means of this instrument in practice. In the statistics given by Mr. Preece, he referred to an accident on the London and Chatham railway in 1871. He (Mr. Rudall) had ascertained that the accident was not occasioned by any defect in the telegraph blocking, but it occurred entirely through the neglect of a Great Northern driver to observe the signal, which he acknowledged was dead against him, and as far as the statistics showed there was not a single case of accident through defect of the working of the signals. He quite agreed with Mr. Spagnoletti with respect to the advantages, for the reasons stated by that gentleman, of the three wire system, and he found it very desirable to have a second or third instrument to fall back upon in the case of accident, or when there was any difficulty with one of the circuits, and they employed men of sufficient intelligence to know when they were out of order. This system employed two wires, and the third wire was used for a speaking instrument, which increased the advantages of the system so far.

Mr. Mackie (responding to the chairman's invitation) said he was not prepared to go into any special details of the system proposed by Col. Binney, but he thought it was one which seemed to possess advantages which, thoroughly worked out, were worth bringing before the meeting this evening. He might state that he had only been acquainted with the apparatus within the last few days, and had not given it so much attention as he should have liked. It seemed to him, in the application of signals on railways for blocking trains, if this plan could be worked out, a great advantage would be derived; and in the early stage of Col. Binney's project, a special line of metal was put in, with which contact was made when an engine ran over it. That made a difficulty in the traffic of the line which might be an objection to its use; but subsequently Col. Binney had carried out experiments to utilise the line itself as a means of communication with the requisite arrangements for working the signal. When the engine went over that part of the line, the motion of the engine completed the circuit, and while the train was running over that portion of the line the signal was worked by it; and he thought the idea of using the line itself as the means of making

circuit, seemed to be of great advantage; it also appeared to him that there were special advantages attached to this system, which were valuable. This was the case especially in regard to one station being ignorant that a signal had been sent from another station, which was notably the case in the serious accident that occurred at Lewisham some years back, through mistake of a signal sent from Blackheath. If the train made its own signal on the line of the railway itself, such an accident could not have occurred. In cases of shunting, and where goods trains were left on the line to be picked up, there were great advantages in this system; because in forming the circuit, as long as there were any carriages on the line the signal must be kept at danger against the approaching train. This system was now, for the first time, before him, but he was told experiments had been made to shew that the circuit could be kept up for two or three miles without fear of breaking down, and that wind and rain would not affect it to a degree to prevent the working of the signal.

A Member enquired how the system was likely to be affected during the period that portions of a railway were submerged by floods?

Mr. Mackie could not answer the question, but reference to that point would lead him to enquire into it.

Colonel Yolland, R.E., said that he had been a strenuous advocate for the working of the whole of the railways throughout the kingdom, on what was improperly called the block system; but which, strictly speaking, was endeavouring to maintain an interval of space between two following trains travelling on the same line. That might be done mechanically, as stated by Mr. Preece, or it might be done more conveniently, better, and at distances varying, by means of the electric telegraph, and he did not hesitate to say the public at large, managers, and superintendents of railways, and the shareholders were largely indebted to the gentlemen who had given so much time, so much patience, and so much intelligence towards the various inventions before them, and for bringing forward these instruments, a great number of which, if not the whole, might be advantageously used for maintaining intervals of space between trains travelling on the same line. He had heard remarks made with reference to the first attempt of the kind made in this country, which he believed was called the permissive system. He believed nobody knew how much good the

permissive system had done towards the avoiding of accidents on the London and North Western system, which was he believed the only railway on which it was maintained for any time. It was impossible to prove this negatively, but it was nothing more or less than a system of cautioning the drivers, an advantage to the greatest extent if properly carried out; but in the course of a short time, as the exigencies of the traffic increased, it degenerated from the original intention, and it resulted in giving caution signals, which in some cases were seen by the drivers, and in other cases were not seen at all. The notion being that the train should be stopped when there was danger, and the driver told what was in front of him, so as to know what to do. Latterly that system broke down, failures were very frequent, and it was with great satisfaction he heard it was now to be given up, to be followed, he was glad to say, by the more perfect one, of what was called the absolute block system, and he must be allowed to remark that an absolute block system alone, which preserved an interval of space would be attended with frequent accidents if the interval of space was narrowed down to 40, 50, 60 or 70 yards, between which the following trains must not pass.

Gentlemen present knew better than he did that it was impossible for an engine driver to be able to pull up a train in a few yards, not knowing the power possessed by the breaks behind him: and if, as was done at the present time by a powerful railway company, it were attempted to maintain what was called the absolute block system, with such intervals of space beyond which the second train must not follow, collisions would occur. The blame would be laid upon the absolute block system, and he did not hesitate to say it was unfair to the drivers, and unfair towards the system itself. It was not giving it a fair chance, and was not what was fairly understood by the preservation of intervals of space.

A great deal of ingenuity had been exercised in preparing the various instruments and apparatus brought before them. He did not propose to enter into the details of them, but he was in favour of the original system of all, and he did not hesitate to say the single needle instrument, properly worked, with simple instructions and the maintenance of proper discipline, might do nearly everything that could be accomplished by the most perfect instrument on the table, and three-fourths of the accidents on the absolute block system were due to want of discipline, disobedience of orders,

going exactly opposite to orders, and in a great number of instances due allowance must be made for the men who did the work. Disobedience of orders was sometimes produced by great anxiety and great zeal to do what they believed their duty in the best possible manner. They were anxious not to stop the traffic. He had only returned at the end of last week from inspecting an accident which might have been attended with most fearful consequences, due to the breakdown of a train, and a succeeding train being permitted to follow it, in consequence of the electric wires being all broken, and the needle, in consequence, was restored to the state in which the sender thought he was communicating with a distant station: and when he found he could get no reply, though he could move the needle, which, according to the system, would be pinned over at the station to which the train was proceeding, when he found he could move the needle he imagined that the men at the distant station at 4 or 5 o'clock in the morning were employed, in accordance with the practice of the line, in attending to their duties, and he sent the train on. The great neglect was in sending it on, in direct disobedience to the instructions, without telling the driver he could not communicate with the station to which he was going. The instructions were explicit for the train not to be sent on, yet it was, and the driver of the following train was not cautioned.

There was another point on which he would offer a few observations, which was this, viz.—what was stated to be the excessive cost of introducing this system upon new lines. It was incorrectly so stated; there was no excessive cost, where the traffic was small, the cost was very little more than the cost of the instrument and putting down the wires. The people who attended to the ordinary traffic were quite capable, with proper instructions, of working the instrument placed in their hands, and doing the work properly, and, as he said, beyond the instrument and wires, the additional cost was merely nominal. On this ground he, for one, was of opinion that no new line of railway should be opened, especially if it were a single line, unless it was worked under two conditions, both of which were adverted to in the papers read, viz.—that there should be something tangible to tell the driver of the train that he was authorised to go on on the single line and need not run the risk of meeting a train coming in an opposite direction. That was done by means of what was called the staff and ticket system. It was well known, that if there were but one

engine in steam on the line there was no necessity for such an institution, because a train could not meet another engine if there was none to meet, and that must be the only train on the line. It could not run into any other, and under such circumstances no block system was wanted unless they required the telegraph to give intelligence to their people, but where there was a large traffic required to be sent on a single line, the practice first introduced by the London and North Western Company of giving the first train a ticket, and so many other trains tickets, and the last train the staff, carried the last of the series of trains along the single line, and when it got to the end of its journey the staff was ready to send off the stream of trains in an opposite direction. That precaution was a tangible thing, it was handed to the driver, shewn to the driver, and gave him proper security, that he might go on ahead. In addition he (Colonel Yolland) held that what was termed the block system was necessary to prevent one of these trains overtaking the other on a bad gradient, in consequence of break-downs, &c., and hence he was an advocate when new lines were opened, where there were not more than 3 or 4 trains daily, that the men should be trained into a proper system; and as the traffic increased they would be able to have all the precautions necessary for working the traffic safely, putting an intermediate station where the distance between the passenger stations might require them and telegraph stations intermediate to diminish the interval.

He had hitherto advocated the single needle instruments, but he did not think they would answer with the traffic of such lines as the Metropolitan, where the trains ran 3 or 4 minutes after each other; and he begged to point out that Mr. Preece had understated the case with regard to the old system of time intervals, because there were many parts in the North of England where superintendents and managers would tell them, though they had not got the block telegraph, and though they had been anxious to have it for years, yet they could not work their traffic if they attempted to maintain an interval of 5 minutes between following trains as a time interval, because they had been lowered to 3 minutes. To revert to the Metropolitan traffic, he had himself heard gentlemen, managers of lines circling round London, say they would not undertake to work the traffic on such lines without the assistance of the absolute block system, as it would be impossible to conduct it with safety. They had got the absolute block system,

and it was to those he saw around him they were indebted for having got it, and he for one felt greatly indebted to them.

Mr. Culley drew attention to the model of an instrument which he believed was the first block instrument used, and was employed in 1845 on a single line in Norfolk, part of what was now the Great Eastern. This he said was a block system, not only between station and station, but over the whole line between Norwich and Yarmouth. Supposing a train was leaving Norwich, Norwich would signal Brandon the terminus, and Brandon would block the needle over, and the other stations would see there was a train on the line. Being a single line then, trains would not be going both ways at the same time. He mentioned this to show that the needle system of block was in existence at a very early date indeed. He went to Derby in 1846, and at that time the Clay Cross tunnel was worked on the block system, which was continued for several years, and he mentioned this to show that it had been many years in existence before it was used on the London and North Western and Metropolitan lines, and was described by Sir W. O'Shaughnessy in his book on Railway Telegraphs published in 1842.

Mr. Ratcliffe said he would endorse Col. Yolland's remark, that where any irregularities occurred on a line where the absolute block system was in use, they were not due to any defect of the system, but to signalmen acting in direct disobedience to instructions. On the line with which he was connected (Great Northern), where the absolute block system was established throughout the main line, not a single accident had occurred through any defect in the working of the system itself.

Mr. Clapp remarked that the only claim he had to say anything on this subject was, that he had studied railway signalling generally, and it occurred to him that the subject he wished to bring before the meeting was connected with signalling; and though no doubt the block system would prevent accidents if it were in general use, yet he believed if another part of the system at present used was not discontinued, the absolute block system would have no good effect. Mr. Preece mentioned in his analysis of railway accidents that there were due to human machinery 40 per cent., and to defective signalling 28 per cent. He would like to know a few more details with reference to defective human machinery. The defect of the signalling would make the human machinery defective. In the block system they had

trains blocked from station to station, but the danger began where the train started. If the trains were motionless, of course no collision could occur: therefore it must be to the system of starting trains they must look for the cause of accident. If they would allow him he would call attention to a subject which was hardly to be believed, but it was a fact nevertheless, that was:—That a system of signalling was at present in use, which had two meanings. A white light was used to start a train, and the same description of light was not only exhibited at the various stations, but was carried about by those engaged at the stations. Some present would, no doubt, remember that two or three years ago an accident happened at Stourbridge. The line was signalled clear; whether it was the absolute block system he could not tell; but a train which had been shunted was started. The driver mistook a naked white light carried on the platform, the train started, and a collision occurred with loss of life and property. Now, if it was possible for an accident to happen through that system, it was equally possible at a future time they might hear of a terrible accident from the same cause. A great deal had been said about the ambiguity of signals, as was instanced in the case of the "Northfleet." Although the accident was not caused by a mistake of signals, yet it was signals having two meanings which prevented assistance being given to the vessel, and the same might happen on a railway in starting a train. He would ask those acquainted with the subject, why not use a red light as a danger signal and a green one for a starting signal, which would throw the responsibility of starting direct upon the driver, whose duty it was to look out for the starting signal, which could not be done under the present ambiguous white light system.

Mr. Chubb said the block system having been in operation on the North London railway for many years, he could speak with some experience on the subject, and he desired to bear his testimony to the benefit which was derived from the use of this system. In early days he tried the system of intervals between trains by time. That was the old system when he commenced railway management, and subsequently he used on that line what was called the permissive block system; but both failed signally, and he adopted another system, and searching for what was best, this appeared the only obvious plan of keeping a distance between trains—a distance of space, and that could only be done by a telegraph arrangement; and

in order to prevent accidents on his line, which was a very difficult one to manage, he adopted this absolute block system, that was as long ago as 1855; and it had been a matter of surprise to him to hear the question raised at the last meeting, as to the desirability of adopting this system on railways in the present day. That question seemed to him to be raised by Mr. Preece's paper. He was not sure that gentleman meant it, but he appeared to stand in the light of an advocate of a system which had appeared to him (Mr. Chubb) for the last 17 years one of necessity, and the further question was raised—whether it was desirable that railway companies should be compelled to adopt that system.

It was upon these two points he wished to say a few words. He had no hesitation in saying that no line with a large and varied traffic could be maintained without proper intervals of space between trains, and he earnestly recommended that system to all whom he addressed. The North London ran trains at intervals of three minutes, but in that respect more difficult than the Metropolitan because there were express trains, and a variety of goods trains which had to be shunted into sidings, and short intervals of passenger trains; and the only way to work that line was by the block system. That system, as he had said, had been in operation on the North London railway for 17 years, and the instruments which were first applied to that system had been working ever since, and they seemed to have been very little referred to in the papers. These were Tyer's instruments, and they seemed adapted, after that long experience, for carrying out the system perfectly. He desired to testify his gratitude to Mr. Tyer for that instrument, and for the simple mode of carrying out the block system he had introduced. He assured them it had not been altered, because it was found best at the beginning, and no alteration had ever been made in it. It was simplicity itself. The men could not talk with it. The men would talk if it was possible, and he found the best way was to let them have a speaking instrument in addition to Tyer's.

One question raised in the paper of Mr. Preece was whether the railways of the country should be compelled to adopt the block system whether they liked or not. He was not so sure they should not, for it seemed to him, having used it for 17 years, a matter of surprise that any persons could be found in the present day to question the desirability of its universal adoption throughout the railways of the

country. He was of opinion, however, that some pressure was needed. He was much struck with the speeches of Mr. Barlow and Mr. Leeman, M.P., at the last meeting. Both those gentlemen congratulated themselves upon the comparatively small amount of mischief that had been done by railway accidents. He (Mr. Chubb) must say that was not his feeling. Accidents had occurred which made one shudder to read the details; sometimes these had occurred from circumstances wholly unavoidable, but at the same time, a great many accidents had taken place which never ought to have occurred at all. He thought if some pressure had been put on, the block system would have been generally adopted years ago. The great increase of traffic pressed it more strongly than anything, but if the screw had been put on the companies years ago, accidents would not have occurred as they had. He had ceased to be a railway manager for 10 years, having retired into the more stupid department of a director, but he spoke from large experience that railway companies do want pressure put upon them for many things. Some had objections to expense in various operations, and they wanted pressure; and on many occasions he had felt grateful to the Railway Department of the Board of Trade for the pressure they had sometimes put upon him. It had brought out improvements which he might not otherwise have thought of. Colonel Yolland had put the screw on sometimes, and he felt grateful for it.

On one occasion he particularly acknowledged the benefit he did. A line of railway joined the North London; it was to be opened for traffic, and Colonel Yolland inspected it. There was a large traffic passing in each direction, and Colonel Yolland imagined something ought to be done more than had been done to protect that junction from passing trains, and said there ought to be a mode by which the points and signals should agree and answer one another, and he said he felt so strongly on this point, that he declined to open the line until it was done, he (Mr. Chubb) thought at the time it was unfair to force this upon him, and he asked Colonel Yolland where it had been done: he did not know, and he was complimentary enough to say he thought if any one could do it, he (Mr. Chubb) could. Here was a line ready for opening, a large traffic waiting, interest of money going on, all of which he pointed out, but Colonel Yolland stuck to his text, and he (Mr. Chubb) found it was necessary to try to comply with it. He picked out an ingenious mechanic from the shop, and told him what

he wanted, and after some three weeks consideration he proposed something whereby the points and signals would answer to one another. The points could not be opened without the signal gave permission. This did not quite do, and they had to start afresh, and after a month a plan was devised with which he believed Colonel Yolland was very pleased, and that was followed by his permission to open the line. He (Mr. Chubb) felt that the man who planned it was deserving of encouragement and he took out a patent for what was now known as Saxby's patent, that gentleman having purchased it of the inventor whose name with Chambers. Here was an instance of the great benefit that resulted from Colonel Yolland's perseverance in his point, and from pressure being brought to bear upon it. He thought pressure was needed. Government railway inspectors were not infallible, but their objects were good ,as he could testify in his own experience. He recommended the block system most earnestly, and he would add, in his opinion Tyer's instrument was superior to anything he saw on the table.

Colonel Yolland, R.E., added, with respect to the Hampstead and City Junction, Mr. Chubb's recollection did not quite serve him, because at the time he (Colonel Yolland) inspected that line, he was aware that the character of the arrangements which had been perfected, was such that contradictory signals could be avoided mechanically; and the signals themselves were placed in the hands of one of the oldest and best signal makers at that time; and he undertook to do what he (Colonel Yolland) suggested, and which the managers acknowledged was desirable. He failed to do so in the two months following, and then it was that Mr. Chubb placed it in the hands of a workman belonging to the North London Railway, who carried it out successfully; though the opening of the line was postponed on that account, as he felt bound to withhold his sanction until that arrangement was carried out. With reference to the subject under discussion, he begged to say that neither himself nor his colleagues interfered to suggest what instruments or what mechanical arrangements should be employed. All they ever contended for was, that such and such things should be done, and effectually done, and it was a matter of ndifference to them by what particular means it was effected.

Mr. W. H. Preece (in reply) said there were two or three points in his paper, which he had hoped would have formed a basis for discussion, but which had been passed over. In the first place

he endeavoured to point out that very great improvements had taken place in the working of railways, and that these improvements had been due not only to the operations of scientific thought, but, in a great measure, to the reports placed in the hands of railway authorities by the Board of Trade. He was sorry that Mr. Barlow should have slightly misunderstood him on this point, inasmuch as that gentleman seemed to imagine that he attributed the improvements that had taken place entirely to the action of the Board of Trade. Having been a railway officer himself, it was not likely he should attempt to ignore what had been done by the companies themselves, for no one knew better than he how railway men had thrown their energies and powers of thought into the adoption of electricity for the purpose of improving the working of the traffic.

Again he pointed out that a very broad distinction should be drawn between the block system as an abstract principle, and the instruments employed to carry out that principle. Nearly all who had spoken had treated the block system as a system of instruments. The railway managers themselves, who gave evidence before the Committee of the House of Lords, had also based all their arguments upon that idea. The block system was *in toto* an abstract principle. It had been ably defined that evening by Col. Yolland as a principle by which trains were kept apart from each other by intervals of space: whether that was obtained by electricity, or by mechanical means, or by optical instruments, as long as trains were kept apart by intervals of space, they had to all intents and purposes a block system. Railway managers had objected to the introduction of the block system on single lines worked by a single engine. According to his definition such lines were fairly worked on the block system, inasmuch as the trains were kept apart by a space which was infinite, and the fact of only a single engine being kept at work formed an effective block. They next came to the staff and ticket system, which was, in fact, the permissive block; but that could, with the greatest ease, be at once made an absolute block, if the station at which the train arrived simply telegraphed back that the train had arrived. It would be a simple thing to use a password to give the station master who sent the train forward, direct and unmistakeable evidence that the train had arrived.

Again they came to lines which managers regarded as not calling

for the exercise of the block system, viz.—branch lines, on which two or three trains ran daily, but even these were worked on the block system, because, if a train went a distance of 10 miles two hours after the other, there must be an interval of space between the two. When they came to branch lines with more frequent trains it was only necessary for light traffic to signal by bells, or such other simple instruments as managers might adopt. Again, when they came to lines where the trains were more frequent, main lines, like that mentioned by Mr. Chubb, more expensive apparatus was required, and the expense of working would be *pro rata* with the traffic on the line; therefore the objection on the score of expense raised to the introduction of the block system on single lines, and lines where the traffic was light was untenable.

He endeavoured in his paper to trace the causes which led to the slow introduction of the block system. Mr. Chubb had told them it had been in use on the North London Railway for 17 years. Mr. Leeman, the other night, told them about its introduction on the North Eastern. A return issued last year showed that upon the 1,400 miles of the North Eastern system only 19 miles were worked on the block system. How was it then, that on the North London it had been in use over the whole line for 17 years, while upon a great traffic like the North Eastern it was only being just introduced? He had stated he believed it was partly owing to prejudice and partly owing to the cost. In the evidence before the Lords' Committee all the possible objections were raised, but nearly every one of them were repudiated by managers themselves. One very great objection adduced was that the block system removed the responsibility from the driver to the signalman, but some of the managers said that was absurd. Mr. Eborall stated that the block system had been used on the South Eastern Railway since 1851, and, in his opinion, the responsibility of the driver was not diminished. In the concluding portion of his paper he had argued as quietly as he could the desirability of making the block system compulsory, and in that conclusion he was confirmed by Mr. Chubb, supported by Colonel Yolland and also by the press. The opinion was that the block system should be made obligatory upon all railways. Mr. Leeman, in his remarks, took his (Mr. Preece's) words in a sense which they were certainly not intended to convey. That gentleman inferred that he stated there was no disposition on

the part of railway boards to listen to the recommendations of their managers. That was not his meaning. He meant to say that the fear of expense often intimidated managers from proposing to their boards expenditure which would not result in increased dividends, and that the expense of the block system was one of the great causes that retarded its introduction, but now that that objection had to a certain extent exploded, the block system was being very rapidly introduced.

It was remarked by Mr. Leeman that they must not regard the block system as infallible; and that accidents would occur notwithstanding its adoption. He (Mr. Preece) said the same, and they knew where human agency was employed, they must suffer occasionally from human error. When he spoke of human machinery he alluded to errors through carelessness and inattention, which frequently formed the cause of accidents. Colonel Yolland spoke of the officers of the Board of Trade as being desirous that the principle should be carried out, without caring about the precise plan on which it should be done, in other words they did not advocate any particular system. Colonel Yolland spoke rather warmly of the needle system, and with the same breath gave the history of an accident he investigated, which proved the defects of the needle system, and which led him (Mr. Preece) to abandon it in preference to another; in other words, they had an account of an accident which arose from the signalman making a mistake in the indication given by the instrument.

There was one cause of error in all absolute electrical block systems which had been lately alluded to, but which was exceedingly important in its bearing on the working of the block system; that was, the liability which all instruments had more or less to be affected by lightning. True, there were systems like Mr. Walker's, which were rarely affected by lightning, or rather had never led to accident from that cause, but that was no proof they were not liable to it. They all knew that currents were generated by inductive influence, and they might be so great as to influence for good or evil the wires and instruments worked by persistent currents, as well as by momentary currents. In fact, in the evidence given before the Committee of the Lords, two managers gave strong evidence in support of what he said, viz.—that lightning was a source of danger. One manager stated that lightning did affect the instrument sometimes, but they provided for that by monthly instructions. If any effect of

lightning were apparent the men acted on the printed instructions in the book. Amongst other instruments thus affected, that of Tyer's was mentioned. They knew, in fact, that the block system did frequently get out of order from atmospheric influences.

There was no doubt lightning did affect needle instruments, and there was also liability of lightning to produce signals which might lead to accident.

Colonel Yolland: There would be no repetition of false signals.

Mr. Preece: It might give such an indication that the man might mistake the signal. He said lightning did affect the instrument, and he begged members who were engaged in adapting electricity to signalling purposes, to look upon this as a greater danger than they did, and endeavour to remedy it; because the obviating of false signals from the effects of atmospheric influences was most deserving the serious attention of telegraph engineers.

There were many applications of electricity to Railway purposes. There were means by which signals were worked at distant stations. Colonel Yolland who, perhaps, more than any other man, had a real affection for the block system, and had done more than any other in endeavouring to enforce its adoption on all railways, had shadowed forth a system of working the block system which it certainly wanted. One effect of working a line like the Metropolitan was, it was possible for a man to give "Line clear" when it was not clear; therefore Colonel Yolland was anxious to introduce a system by which the train itself should clear the line, or make such preparations as to clear the line. That is a problem which had been taken up by one or two persons, and it was a point to which those who devoted their attention to block signalling should aspire. He fully agreed with what Mr. Spagnoletti said, that there were immense advantages in the three-wire system and persistent current over the single wire; and, probably, but for economical considerations, the single wire would not have held its own; but it had been so improved, altered and enlarged, that many of the defects attributed to it had now been removed. They could not hope to eliminate all the faults which the three wires eliminated, but as an electrical apparatus for working the block system it was altogether a very perfect one. He was very much gratified with the remarks of Mr. Chubb, as they shewed that all railway men were not disposed to ignore the experience of others, and, on the other hand, if railway men believed in the ex-

perience of other lines, there would not have been the same delay in the introduction of such an improvement as the block system undoubtedly was.

The CHAIRMAN said the hour was advanced and he had to bring this discussion to a close. He would not trouble the meeting with many remarks of his own. He might congratulate the Society upon having this subject so well brought before them by men so eminently qualified to deal with it in all its branches, not only in the papers by Mr. Preece and Captain Mallock, but in the various instruments before them, designed to meet the difficulties of the case in various ways. They had on the table instruments representing, as they might say, the English system, and they had instruments representing the system adopted on the continent, and more particularly in Germany. Of course there were points of contrast between both, but if they drew a distinction, it seemed to him to be this — that in the English system, the bell or acoustic system was adhered to, whereas in Germany that principle was expressly excluded as being less reliable than the optical combined with the electrical system. The whole subject had led, he thought, to very satisfactory conclusions arrived at from this discussion. Those conclusions seemed to point to a general conviction that the absolute block system was the most satisfactory arrangement, whichever way it might be carried out. They had the evidence of Colonel Yolland, who had been so useful and powerful an advocate of that system from the first, and he thought nothing could be added to his remarks on that subject; only he (the Chairman) was strongly of opinion that if the block system was adopted, it ought to be an entire block. A block system which left anything to the men to alter, was no block system at all. It must have the invariable effect of throwing the men off their guard and must produce danger. The block system, therefore, ought to be complete in itself, and there should be no exception permitted in its exercise. What system might be best to enable trains to follow each other in such rapid succession as was done on the Metropolitan line he was not prepared to say, but the space system seemed to him the most perfect; at the same time even the interval of distance was not always a safeguard against collisions, and he could himself give an illustration of that. Mr. Preece had said, that where only a single engine was on a line, no collision could arise, he (the Chairman) happened to know a case in which a collision did occur, a collision of carriages

passing in opposite directions, though there was only one engine on the line, and of course only one train. This happened on a line near Paris where the entrance to the station formed a circle, and on one occasion, the train being longer than usual, the engine of the train having gone round the circle ran into the end carriage of the train it was drawing; and thus showed the difficulty of devising a system applicable to all circumstances. He now begged to propose a vote of thanks to Mr. Preece and Captain Mallock respectively, for their valuable communications.

The proposition was unanimously agreed to, and the meetings for the Session were brought to a close.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :—

Capt. V. Hoskiar	Copenhagen.
Emile Lacoine	Constantinople.
J. B. Stearns	Boston.
Laurence Trant	Buenos Ayres.
Peter Trant	Rosario.

As Member :—

Lieut. Colonel Parsons, R.E.	Southampton.

As Associates :—

John Faulkner	Manchester.
Lewis Incledon	General Post Office.
Andrew Jamieson	Charlton.
S. C. Tisley	Brompton Road.

As Student :—

Henry McKain	2, Westminster Chambers.

ORIGINAL COMMUNICATIONS.

ON THE STRENGTH OF CYLINDRICAL WROUGHT IRON TELEGRAPH POLES.

By F. C. Webb, M. Inst. C.E., M.S. Tel. E.

As the subject of the strength of iron telegraph poles seems, from a statement made in a recent paper, scarcely understood, the following may, perhaps, be of some service.

Mr. Edwin Clark, in his work on the Britannia Bridge, gives the details of some experiments on cylindrical wrought iron beams, from which the following formula has been deduced.

$$w = \frac{a\,d\,c}{l}$$

Where w is the breaking weight applied at centre of beam,
d the diameter of the tube,
a the area of the whole section of the iron,
l the length between supports, and
c a constant determined by experiment representing the strain per square inch, and which was found for cylindrical beams to be 13 tons.

From the above it is evident that—

$$a = \frac{w\,l}{d\,c}$$

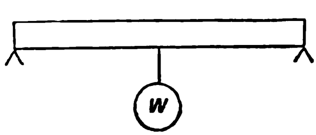
Now the area of section in the case of thin tubes, may be very approximately taken as equal to $d\,\pi\,t$ where d is the diameter of the tube, π equals 3·14, 16 and t is the thickness of plates of the tube.

$$\text{Therefore } d\,\pi\,t = \frac{w\,l}{d\,c}$$

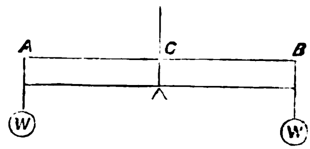
From which we get—

$$t = \frac{w\,t}{d^2\,\pi\,c}$$

Now if we take a beam that will break with a given weight w at its centre (Fig. 1.)



Then if we poised it on its centre (Fig. 2.)



it will just break with half the weight applied at each end.

Now a pole may be taken as a cantilever, and as such is the same as half the beam in figure 2, that is to say, C B may represent the pole, the portion buried in the ground holding the place of, and performing the duty of the half beam A C and its attached weight.

Therefore, if we take any pole and multiply its length out of the ground by 2, and the strain at its top end by 2, we can apply the formula for beams, or—

$$t = \frac{2\,w\,2\,l}{d^2\,\pi\,c}$$

Taking a pole 32 feet long, of which 6 feet is buried in the ground, and 2 feet diameter with a strain produced by the wires at the top, acting at right angles to its axis of $2\frac{1}{4}$ tons, we have a cantilever 26 feet long, and applying the above formula, we have—

$$2\ l = 52 \text{ feet} = 636 \text{ inches.}$$
$$2\,w = 4{\cdot}5 \text{ tons.}$$
$$d = 24 \text{ inches.}$$
$$c = 13 \text{ tons.}$$

$$\text{Therefore } 2\,w\,2\,l = 636 \times 4{\cdot}5 = 2862, \text{ and}$$
$$d^2\,\pi\,c = 24^2 \times 3{\cdot}14 \times 13 = 23504, \text{ and}$$
$$\frac{2\,w\,2\,l}{d^2\,\pi\,c} = \frac{2862}{23504} = 0{\cdot}12 \text{ of an inch}$$

as the thickness for breaking strain, and multiply this by 3 as a co-efficient of safety, we get 0·36 inches as a safe thickness.*

And in the same way for a pole 12″ diameter and of similar length, we get 1·44 inches, or nearly $1\frac{1}{2}$ inches.

* Major Webber, in his recent paper, makes the thickness as much as 6 inches. This I pointed out to him after the discussion on his paper, and showed him my formula and result, but it still stands as he first stated it, printed.

ON THE PERCENTAGE OF AVERAGES.

By W. H. Preece, Member Inst. C.E. and Soc. T.E.

Having recently had occasion to tabulate some returns of the work done in various Postal Telegraph Offices, and to compare the increase and decrease during the different months and years, I was surprised to find, on checking the average numbers, that I never could get the *average percentage* to agree with the actual percentage. It will be seen in the following table that while the *actual percentage* of the average increase of six months of 1871 over six months of 1870 was 39·7, the *calculated average* per cent. (by adding up the column and dividing it by the number of entries) was 38·5.

			Increase.	
Months	1870.	1871.	Number.	Percentage.
April ...	301	324	23	7·635
May ...	295	358	63	21·35
June ...	317	396	79	24·9
July ...	306	520	214	69·9
August ...	373	608	235	63·0
September	354	512	158	44·5
Average per month	324·3	453	129	39·7
			Calculated	(38·5)

In every case tried (except the *one* case of all the numbers in column marked 1870 being equal) the two numbers always disagreed. I tried to discover the cause of this by the following algebraical investigation:—

Let

$$\left.\begin{matrix} a, & b, & c, & \ldots\ldots \\ a', & b', & c', & \ldots\ldots \end{matrix}\right\} \text{ be two series of numbers,}$$

$$d, \ d', d'', \ \ldots\ldots \quad \text{their differences:}$$

then $$\left.\frac{100d}{a}, \quad \frac{100d'}{b}, \quad \frac{100d''}{c},\right\} \text{ will be the percentage of these differences.}$$

Arranging them as a table, we have:—

Numbers.	*Differences.*	*Percentages.*
a	d	$\frac{100d}{a}$
b	d'	$\frac{100d'}{b}$
c	d''	$\frac{100d''}{c}$
.	.	.
.	.	.
.	.	.

The averages of which will be:—

$$\frac{1}{n}\left\{a+b+c\ldots\right\} \quad \frac{1}{n}\left\{d+d'+d''\ldots\right\} \quad \frac{100}{n}\left\{\frac{d}{a}+\frac{d'}{b}+\frac{d''}{c}\ldots\right\}$$

But the actual percentage x is

$$\frac{1}{n}\left\{a+b+c\ldots.\right\} \;:\; \frac{1}{n}\left\{d+d'+d''\ldots.\right\} \;::\; 100 \;:\; x$$

or,

$$x = \frac{\frac{100}{n}(d+d'+d''\ldots)}{\frac{1}{n}(a+b+c\ldots.)}$$

$$= \frac{100\,(d+d'+d''+\ldots.)}{a+b+c+\ldots.} \quad \ldots. \; (1)$$

And this does *not* equal the calculated percentage

x' or $\frac{100}{n}\left\{\frac{d}{a}+\frac{d'}{b}+\frac{d''}{c}+\ldots\right\}$ .... (2) *unless* a, b, c ..... are all equal.

In which case

$$x = 100\,\frac{(d+d'+d''+\ldots.)}{n\,a} = \frac{100}{n}\left\{\frac{d+d'+d''+\ldots.}{a}\right\} = x'$$

When a, b, c are *not* all equal we have—

$$x'-x = \Delta = 100\left\{\frac{1}{n}\left(\frac{d}{a}+\frac{d'}{b}+\frac{d''}{c}+\ldots\right)-\frac{d+d'+d''+\ldots}{a+b+c+\ldots}\right\}\ldots(3)$$

Also when $d = d' = d'' = \ldots..$ we have, as a particular case of Eq: (3):—

$$x' - x = \Delta = 100 \left\{ \frac{d}{n} \left(\frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \cdot\cdot \right) - \frac{n\,d}{a + b + c + , . .} \right\} \ldots (3^A)$$

Examples :—

(I.) Where $a = b = c = \ldots.$ Here $x = x'$

	Numbers.		Differences.	Percentages.
	436	– 464	28	6·42
	436	– 508	72	16·5
	436	– 709	273	62·5
	436	– 450	14	3·21
	436	– 836	400	91·7
	436	– 652	216	49·5
Averages	436	603·1	167·1	38·3 $= x'$

$$\text{or } x = \frac{100.\ 167{\cdot}1}{436} = 38{\cdot}3 = x'$$

(II.) Where $a, b, c \ldots.$ are *not* all equal:—as a simple case of Eq. (3) we take $d = d' = d'' = \ldots..$

Here x does *not* $= x'$

Numbers.		Differences.	Percentages.
336	367	31	9·22
421	452	31	7·35
318	349	31	9·74
100	131	31	31·00
760	791	31	4·075
354	385	31	8·75
6 | 2289	6 | 2475	181	6 | 70·135
381·5	412·5	31	11·689

$\therefore\ x' = 11{\cdot}689$

$$\text{But } x = \frac{(412{\cdot}5 + 381{\cdot}5)\ 100}{381{\cdot}5}$$

$$= 8{\cdot}125$$

$\therefore\ x' - x = \Delta = 3{\cdot}56$

showing a difference of 3·56 per cent. between the two results.

If now we apply the formula (3)

$$x' - x = \Delta = 100 \left[\frac{1}{n} \left(\frac{d}{a} + \frac{d'}{b} + \frac{d''}{c} + \cdots \right) - \frac{d + d' + d'' + \ldots}{a + b + c + d} \right]$$

we have

$$d = d' = d'' = \ldots = 31$$
$$n = 6$$

$$a = 336$$
$$b = 421$$
$$c = 318$$
$$d = 100$$
$$e = 760$$
$$f = 354$$

and by *formula* (3) or the simplified form (3^A)

$$\Delta = 100 \left[\frac{31}{6} \left(\frac{1}{336} + \frac{1}{421} + \frac{1}{318} + \frac{1}{100} + \frac{1}{760} + \frac{1}{354} \right) - \frac{186}{2289} \right].$$

$$= 100 \left[5{\cdot}16 \, ({\cdot}00297 + {\cdot}00237 + {\cdot}00314 + {\cdot}01 + {\cdot}001315 + {\cdot}00282) \right.$$
$$\left. - {\cdot}08125 \right]$$
$$= 11{\cdot}6842659 - 8{\cdot}12581$$
$$\Delta = 3{\cdot}56 \text{ approximately,}$$

a result which agrees with the one obtained from actual calculation. It is therefore evident that, in drawing comparisons from any returns, we cannot average the percentages, but we must per centage the general averages.

ON "LIGHTNING PROTECTORS."

By John Fletcher.

As "lightning conductors" protect the coil wires from fusion, so the "induced coils" protect the instruments from interruption, by preventing the needles from being demagnetized.

The strength of a current from a flash of lightning may not be sufficiently great to fuse the wire, or its potential high enough to dart across to the conductor, yet it may demagnetize, and consequently cause an interruption; so that these non-demagnetizable coils might

justly be called "lightning protectors." Before treating on the latter I have a "lightning conductor" to suggest (as shown in Figs 1 and 2),

Fig 1

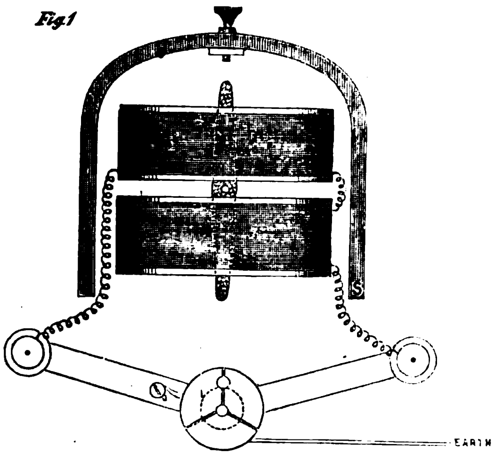
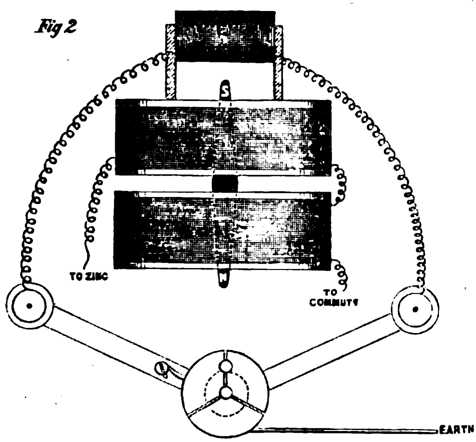


Fig 2



simpler, and, I think, more efficient than any I know. Instead of a boxwood reel, or reed, as at present, a brass reel should be used, to be divided into three parts, and insulated by space from each other. Two of these parts to be connected by brass plates, or thick wire, to the terminals holding the coil and line wires, and the third part (at intermediate stations) to earth. A single No. 22 wire, well covered with silk, to be wrapped several times round this divided pulley, and secured to one side only by a screw. I look upon this as a more efficient protector, since the probability is the lightning would

go to earth and not pass through more than two instruments, and a simpler one, because it could be more readily replaced, and that without at all disconnecting the circuit. This same brass divided pulley might serve as a peg-switch to cut, either the coils out of circuit only, or put to earth during heavy thunderstorms.

In returning to induced coils, up to the present time, the "soft-iron needle" has been the fundamental principle. In June of 1865, Mr. C. F. Varley, upon seeing two sketches of soft-iron needles, induced by a permanent magnet, as per Figs. 3 and 4 (although he condemned Fig. 3), said the idea was new, and that Fig. 4 deserved a trial.

Fig 3.

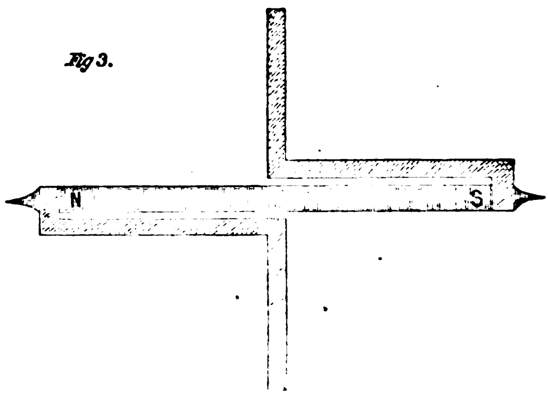
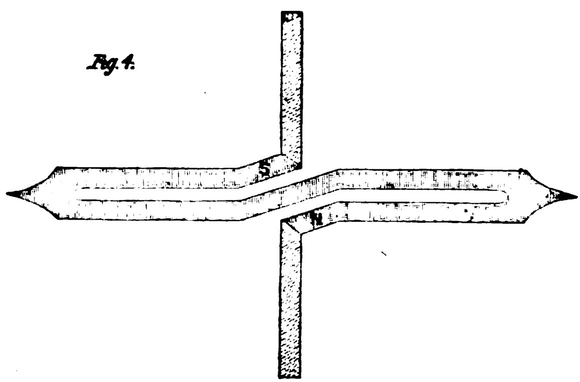


Fig. 4.



These induced or non-demagnetizable coils, as they are called, have taken various forms, under different patents, but they have all soft iron needles. I believe the first to be patented was that of Mr. S. A. Varley; then follow Brittan's, Tyer's, and Spagnoletti's.

It has been said that Varley's was objectionable on account of the attraction of the permanent magnets, causing too much friction on

the pivots, being in one direction only. Subsequent forms were intended to overcome this. In theory it may appear to be so, but in practice it is doubtful. The remedy would be worse than the evil, unless the needle, with its axle, be exactly fixed, both as to horizontal and vertical distances, between and within the magnets, to say nothing of getting two magnets of equal power. Too much friction on the pivots is undoubtedly the drawback to all these schemes. In addition to a soft-iron needle, Brittan's has a soft-iron core. But this is still more objectionable, from the fact that the residuary magnetism in the core prevents rapid working; therefore it is not suitable for speaking instruments, although it might do for instruments where slow working only is required, as in "block telegraphs." The foregoing should be called, not "non-demagnetizable" or "induced coils," but "magnetically induced needles."

Still keeping to the soft-iron needle, but overcoming the objection of too much friction on the pivots, I will now describe an "electrically induced needle" (as per Fig. 1) which I made, and which I worked satisfactorily on a circuit of 120 miles, and I am told it worked well on a circuit of over 200 miles. The same coils are used as in an ordinary speaking instrument, but are placed horizontally instead of vertically, the soft-iron needle being magnetized by the current, and the permanent magnet attracts and repels it to the right or left, according to the direction of the current through the coils. This scheme has in its favour that it can be easily adapted to the present coils, and as the permanent magnet may be shifted to the right or left, for which a slot and set-screw are provided,—earth currents (deflections)—may be compensated for, and messages read, when they otherwise could not.

Another form which I have thought would work well, but which I have not yet tried, is shown in Fig. 2. The coils are fixed horizontally, as in the previous scheme, but the needle is of steel, and the permanent magnet is substituted by a coil having a soft iron core. This electro-magnet forms the line circuit for receiving signals, and the other (ordinary) coils are placed in the battery circuit so as to remagnetize the needle each time the commutator is moved, whether in sending a message or acknowledging a signal. Whether the residuary magnetism objection will hold good in this, as in Brittan's, remains to be tried.

EQUATIONS CONNECTED WITH TELEGRAPH WIRE.

To the Honorary Secretary,
Society of Telegraph Engineers.

Dear Sir,

As the enclosed paper of equations (issued to the Indian Government Telegraph Department in 1866) might form a sequence to Lieut. Jekyll's paper on strains at angles, &c., I forward them in case the Council may consider them worth publishing together with it.

I again beg to point out to the Society how weight and not Birmingham wire gauge enters into every calculation connected with wire, and that, therefore, multiples of a unit of weight are the natural telegraph wire gauge (B.W.G.)

Yours truly,
H. MALLOCK.

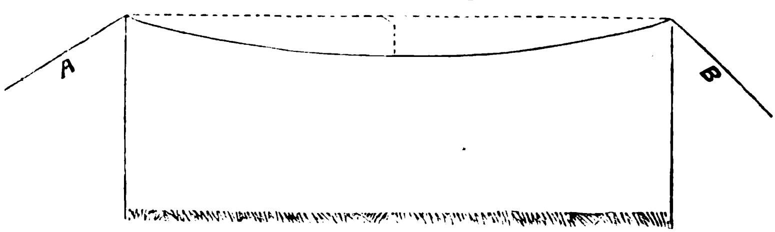
The first equation is for the various demands of a span and from it the others have been deduced.

The second, for the dip of a span.

The third, for the strain on an insulator at an angle.

The fourth, for the opening of an angle, where the breaking strain of an insulator, the weight of wire, and the distance of the supports are known.

First—To find the strain at points of suspension.



Let, $A\ B = a$ in yards.
Dip $= x$ in yards.
W = weight of 1 yard of wire in lbs.
T = strain of points of suspension.

then, $T = \left\{ \frac{a^2}{8\,x} + \frac{7}{6}\,x \right\} W$*

* *Vide* Gregory's Mathematics on the Catenary.

Second—To find the dip the wire should have.

$$T = \left(\frac{a^2}{8\,x} + \tfrac{7}{6}\,x\right) W$$

$$\frac{T}{W} = \frac{3\,a^2 + 28\,x^2}{24\,x}$$

$$\frac{T}{W} \times 24\,x = 3\,a^2 + 28\,x^2$$

$$28\,x^2 - 24\,\frac{T}{W}\,x = -\,3\,a^2$$

$$x^2 - \frac{24\,\frac{T}{W}\,x}{28} = -\,\tfrac{3}{28}\,a^2$$

$$x^2 - \tfrac{6}{7}\,\frac{T}{W}\,x + \left(\tfrac{3}{7}\,\frac{T}{W}\right)^2 = \left(\tfrac{3}{7}\,\frac{T}{W}\right)^2 - \tfrac{3}{28}\,a^2$$

$$x - \tfrac{3}{7}\,\frac{T}{W} = \sqrt{\left(\tfrac{3}{7}\,\frac{T}{W}\right)^2 - \tfrac{3}{28}\,a^2}$$

$$= -\,\sqrt{\tfrac{9}{49}\,\frac{T^2}{W^2} - \tfrac{3}{28}\,a^2}$$

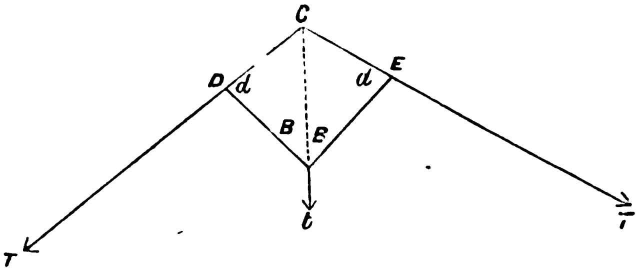
$$= -\,\sqrt{\frac{36\,\frac{T^2}{W^2} - 21\,a^2}{196}}$$

$$= -\,\frac{\sqrt{36\,\frac{T^2}{W^2} - 21\,a^2}}{14}$$

$$x = \tfrac{1}{7}\left(\frac{3\,T}{W} - \sqrt{\frac{36\,\frac{T^2}{W^2} - 21\,a^2}{2}}\right)$$

N.B.—In working out this, it must be remembered that the breaking strain of wire is calculated at a certain number of lbs. per square inch of section, or at a certain multiple of its own length, and that the weight of a yard will vary as the sectional area. Hence $\frac{T}{W}$ will be a constant number for any size. This number is 6,600 for strength of best wire, and 4,400 being ⅔rds of this, may be used where it is of an object to reduce the dip. But, where height is of less importance, it will be safer to use 3,300 or half the breaking strain.

Third—To find the strain on an Insulator at an angle.



Let, $C\,D$ and $C\,E$ be two Telegraph wires.

T, the strain on them.

t resultant strain at C.

$\theta =$ angle $D\,C\,E$.

$B =$ angle $t\,C\,D = \dfrac{D\,C\,E}{2} = \dfrac{\theta}{2}$

Here as,

$$T : t :: \sin. B : \sin. D.$$
$$:: \sin. B : \sin. (180-2\,B.)$$
$$:: \sin. B : \sin. 2\,B.$$
$$:: \sin. B : 2 \sin. B \cos. B.$$
$$:: 1 : 2 \cos. B.$$
$$t = 2\,T \cos. B.$$
$$= 2\,T \cos \frac{\theta}{2}$$

Fourth—To find the angle included by two wires, the strength of the insulator being fixed.

From No. 3 $\quad \cos. \dfrac{\theta}{2} = \dfrac{t}{2\,T}$

$$= \frac{t}{2\,w\left\{\dfrac{a^2}{8\,x} + \frac{1}{6}\,x\right\}}$$

But in practice where in lengths of $\frac{1}{16}$ mile the dip is not more than 2 feet $\frac{7}{8}$, x will be immaterial in respect to the other terms of the Equation, and may be disregarded when the simplified Equation will stand.

$$\cos.\ \frac{\theta}{2} = \frac{t}{2\ w\left(\frac{a^2}{8\ x}\right)}$$

$$= \frac{4\ t\ x}{w\ a^2}$$

The following is an example of the last Equation worked out by a possible case.

Let, $t = 750$ lbs. $=$ { half breaking strain of a Douglas angle Insulator tested by breaking the wire suddenly on one side.

$x = 2$

$w = \cdot 625 =$ weight of 1 yard No. 1 wire.

$a = 110 = \frac{1}{16}$ mile.

$$\text{then,}\quad \cos.\ \frac{\theta}{2} = \frac{4 \times 2 \times 750}{\cdot 625 \times 12{,}100}$$

$$= \frac{8 \times 75}{6{\cdot}25 \times 121}$$

$$= \frac{6}{7{\cdot}5625}$$

$$\log.\ 6 = 0{\cdot}778151$$

$$\log.\ 7{\cdot}5625 = \frac{\bar{1}{\cdot}878666}{\bar{1}{\cdot}899485}$$

$$\log.\left(\frac{6}{7{\cdot}5625}\right) = \bar{1}{\cdot}899485$$

$$= \log.\ \cos.\ 37^\circ\ 30''$$

$$\therefore\ \text{angle}\ \theta = 75^\circ$$

TABLES TO FACILITATE THE CALCULATION OF STRAINS OF OVERHEAD LINE WIRES.

TABLE I.

STRAINS ON SUSPENDED (IRON OR STEEL) WIRES.

Col. 1.			Col. 2.	Col. 3.
Relation of Sag to Span. H.	:	L.	Proportional Strain. K.	Proportional Length. (Span = 1) L.
1	to	10	4·67	1·02667
1	,,	15	6·62	1·01185
1	,,	20	8·61	1·00667
1	,,	25	10·74	1·00427
1	,,	30	12·83	1·00297
1	,,	35	14·92	1·00218
1	,,	40	17·02	1·00167
1	,,	45	19·11	1·00132
1	,,	50	21·21	1·00107
1	,,	60	25·43	1·00074
1	,,	70	29·64	1·00054
1	,,	80	33·86	1·00042
1	,,	90	38·07	1·00033
1	,,	100	42·22	1·00027
1	,,	110	46·52	1·00022
1	,,	120	50·74	1·00019
1	,,	130	54·96	1·00016
1	,,	140	59·18	1·00014
1	,,	150	63·40	1·00012
1	,,	160	67·63	1·00010
1	,,	170	71·85	1·00009
1	,,	180	76·07	1·00008
1	,,	190	80·30	1·00007
1	,,	200	84·52	1·00006
1	,,	225	95·08	1·00005
1	,,	250	105·64	1·00004
1	,,	275	116·21	1·00004
1	,,	300	126·77	1·00003
1	,,	350	147·89	1·00002
1	,,	400	169·02	1·00002
1	,,	450	190·14	1·00001
1	,,	500	211·26	1·00001

Rules for using the above Table.

I.—*The span and sag being given to find the absolute strain per square inch section.*

Rule.—In column 1, find the nearest relation of sag to span, and opposite to it (in col. 2) the proportional strain (K). The absolute strain in lbs. is K times the span in feet.

Example.—If a steel wire were to be stretched across a river (span 2,340 feet), and on account of shipping, only 39 feet sag could be allowed, what would be the absolute strain?

$$39 : 2340 :: 1 : 60.$$

In the table opposite the proportion 1 : 60 is given (col. 2) the proportional strain (K)=25·43lbs. The absolute strain is, therefore, 25·43 × 2,340 = 59,506lbs. per square inch.

II.—*The span and sag being given, and also the sectional area of the wire in square inches, to find the actual strain in lbs.*

Rule.—In column 1, find the nearest relation of sag to span, and opposite to it (in col. 2) the proportional strain (K). Then the actual strain in lbs. is—

$$K \times \text{area of wire} \times \text{span in feet.}$$

Example.—Span of 111 yards with a sag of 20 inches (=1 to 200). Required the actual strain of a No. 11 B.W.G. iron wire? In col. 1, opposite 1 : 200, we find K=84·52. The sectional area of No. 11 wire is = 0·0125 sq. inch. Therefore the actual strain will be—

$$84{\cdot}52 \times 0{\cdot}0125 \times 333' = 352 \text{ lbs.}$$

III.—*The span and sag being given to find the length of the suspended wire.*

Rule.—In column 1, find the nearest relation of sag to span: col. 3 gives the proportional length (L). The actual length is = L times the span in feet.

Example.—Span 200 feet; sag 10′ = (1 : 20). The proportional length of which is 1·0067. Therefore, length = 1·0067 × 200 = 201·34 feet.

IV.—*The span and maximum actual strain of a wire being given to find the necessary sag.*

Rule.—Divide the given maximum strain in lbs. by the area of the wire in inches multiplied by the span in feet, and with the quotient refer to col. 2 of the table for the nearest value. Opposite to this in col. 1, you will find the sag.

Example.—It is wished to stretch a No. 8 steel wire across a space of 2,000 feet in such a way that the maximum strain upon it will not exceed 900 lbs. The area of No. 8 wire is 0·023 sq. inch, and

$$\frac{900}{\cdot 023 \times 2000} = 19{\cdot}565$$

In col. 2 the nearest value to 19·565 is 19·113 corresponding to a sag of 1 : 45. The sag will, therefore, have to be—

$$\frac{2000}{45} = 44\tfrac{4}{9} \text{ feet.}$$

V.—*Two neighbouring spans of different lengths which do not form an angle at the Post being given, to find the sag which one wire must be allowed in order to balance the strain of the other on the common support.*

Rule.—Having found the actual strain of the first section, the sag of the second is calculated by the Rule IV. given above.

Example.—A span of 2,000 feet is crossed by a No 8 wire (area = 0·023 sq. inch) having $44\frac{1}{2}$ feet sag (1 : 45), and, therefore, an actual strain of

$$19{\cdot}11 \times 0{\cdot}023 \times 2000 = 879 \text{ lbs.}$$

The neighbouring span is only 400 feet—

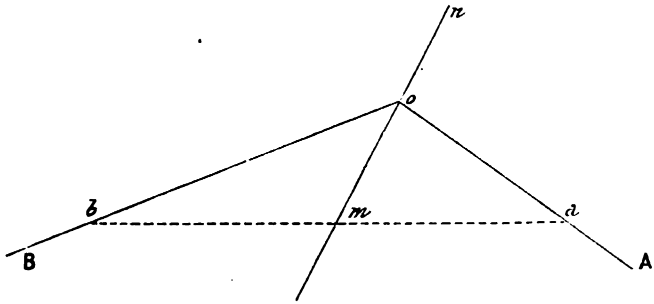
$$\frac{879}{\cdot 023 \times 400} = 95{\cdot}5 \quad \text{(See Rule IV.)}$$

In col. 2 the nearest value to this is 95·08, corresponding to a sag of 1 : 225. The wire must, therefore, be pulled up till the sag is—

$$\frac{400}{225} = 1{\cdot}8 \text{ feet or about 21 inches.}$$

VI.—*Two neighbouring spans having different strains, and making an angle at the Post being given, to find the proper direction for a strut or stay.*

Rule.—Find by the foregoing rules the two strains. Measure off from the pole in the direction of each wire, a length proportional to its strain. Join the ends of the measured lengths by a straight line, the middle point of which sighted towards the post gives the proper line of strut and stay.



EXAMPLE.—From the post O, are two wires. The strain of A is 600 lbs., that of B is 900 lbs.

I measure off in the direction OA, a length of 10 feet to *a*; and in the direction OB, a length of 15 feet to *b* (because 10 and 15 are proportional to 600 and 900). The points *a* and *b* I then join by a straight line, *a b*, the middle of which is at *m*. Therefore, a strut should be placed in the line *m* O. If I want a stay, I sight the line on the other side of the Post towards *n*, and place the stay in the direction *n* O.

VII.—*The span, sag, and temperature (in deg. centigrade) of a wire being given, to calculate the effects of an alteration of temperature.*

RULE I.—Take from col. 3 of the table the proportional length corresponding with the given span and sag.

II.—Then $\left\{\begin{matrix}\text{increase}\\ \text{decrease}\end{matrix}\right\}$ this proportional length by $0\cdot\dot{0}012$ °/₀ for every degree centigrade which the altered temperature is $\left\{\begin{matrix}\text{above}\\ \text{below}\end{matrix}\right\}$ the original temperature.

III.—Lastly, with the altered proportional length, find (in col. 3) the nearest value, and in col. 1, the corresponding sag. The proportional strain in col. 2 multiplied by the span in feet, gives the resulting strain in lbs. per square inch of section.

EXAMPLE.—An iron wire was stretched across a span of 400 feet, with a sag of 5 feet (1 : 80) at a temperature of 25° C. The absolute strain upon it was therefore $400 \times 33\cdot86 = 13{,}544$ lbs. per square inch. In winter during a frost of—5° C. what would the sag and strain become?

1.—By Col. 3 we find that the proportional length for sag 1 : 80 would be 1·00042.

2.—The difference of temperature ($+25°$ to—$5°$ C.) would be $30°$ and $30 \times 0·0012$ % $= 0·036$ %. Then $1·00042 - 0·036$ % $=$ $1·00042 - 0·00036 = 1·00006$.

3.—In Col. 3 we find the proportional length 1.00006 corresponding to a sag of 1 : 200, and (Col. 2) a strain of about 85 lbs. $85 \times 400 = 34,000$ lbs. per square inch, which would be sufficient to permanently stretch the wire.

VIII.—*To find the actual* $\left\{ \begin{array}{c} \textit{stress} \\ \textit{strain} \end{array} \right\}$ *on a given* $\left\{ \begin{array}{c} \textit{strut} \\ \textit{stay} \end{array} \right\}$ *placed in the proper direction.*

Rule 1.—Mark out by the method given in Rule VI. the line of direction, and measure the distance *m* O, in feet. The horizontal strain on the post in the direction *n m*, is equal to the actual strain on A in lbs., multiplied by twice *m* O, in feet, and divided by O *a*, in feet.

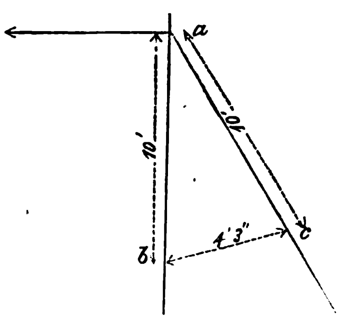
2.—Next measure downwards and mark from the point where the $\left\{ \begin{array}{c} \text{strut} \\ \text{or stay} \end{array} \right\}$ joins the post, a length of 10 feet, on each; and, with a tape, measure the distance across in feet, between the two marks.

3.—Find in Table II. (Col. 1) the nearest value to this distance, and, with the corresponding figure in (Col. 2), multiply the horizontal strain found (in 1) above. The product is the actual strain on the $\left\{ \begin{array}{c} \text{strut} \\ \text{or stay} \end{array} \right\}$ in lbs.

Table II.

STRAINS ON STRUTS OR STAYS.

Col. 1.		Col. 2.
Distance between Strut or Stay, and Post 10 feet down.		Proportional strain on Strut or Stay.
Feet.	Inches.	
2	0	5·02
2	3	4·47
2	6	4·03
2	9	3·67
3	0	3·37
3	3	3·12
3	6	2·90
3	9	2·71
4	0	2·55
4	3	2·41
4	6	2·28
4	9	2·17
5	0	2·07
5	6	1·89
6	0	1·75
6	6	1·63
7	0	1·53
7	6	1·44
8	0	1·36
8	6	1·30
9	0	1·24
9	6	1·20
10	0	1·16



Example.—The post in the example given with Rule VI. is provided with a stay in the line *m* O. The length of the line *m* O, by measurement I find to be 6′. The horizontal strain on the Post is therefore

$$\frac{600 \times 2 \times 6}{10} = 720 \text{ lbs.}$$

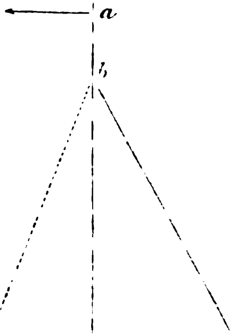
I next mark off from the point (*a*) 10 feet (to *b* on the Post and to *c* on the stay) and measure the distance across from *b* to *c*, which I find to be 4′ : 3″.

Lastly, I refer to Table II., and opposite 4′ : 3″ I find 2·41.

The strain on the stay is therefore

$$2{\cdot}41 \times 720 = 1735 \text{ lbs.}$$

In this rule it is supposed that the strut or stay touches the Post at the same height as the mean strain acts horizontally.



If, however, the strain acts higher or lower than the top of the strut or stay, the result found by the last rule must be multiplied by the height of the strain, and divided by the height of the top of the strut or stay, both being measured on the Pole.

EXAMPLE.—Suppose in the last example the mean strain acts at 20 feet from the ground, but that the stay reaches only 15 feet up, then the actual strain will be

$$\frac{1735 \times 20}{15} = 2313 \text{ lbs.}$$

IX.— *The resultant horizontal strain of the wires in the proper direction of a* $\left\{\begin{matrix}\textit{strut} \\ \textit{or stay}\end{matrix}\right\}$ *and the maximum strain which the* $\left\{\begin{matrix}\textit{strut} \\ \textit{or stay}\end{matrix}\right\}$ *is to bear being given, to find the angle at which it must be placed against the Post.*

RULE —(1). Find by Rule VIII. (1) the horizontal strain in lbs., acting in the proper direction of the $\left\{\begin{matrix}\text{strut} \\ \text{stay.}\end{matrix}\right\}$

(2). Divide the given maximum strain in lbs. by the horizontal strain in lbs., and find in col. 2, Table II, the nearest value to the quotient; col. 1 then gives the distance which the $\left\{\begin{matrix}\text{stay} \\ \text{strut}\end{matrix}\right\}$ must stand from the Post at a point measured 10 feet down on each.

EXAMPLE.—Suppose the horizontal strain calculated by Rule VIII. (1) to be, as in the last example = 720 lbs.; and that a stay which must not bear more than 1,500 lbs., is to be used—

$$\frac{1\,500}{720} = 2\cdot 08.$$

In Col. 2, Table II., the nearest value to this is 2·07, and in Col. 1 the corresponding distance between the 10 ft. marks is given, = 5 ft.

In dealing with overhead telegraph wires, the *actual* strain may be, without sensible error, taken as the horizontal strain, because they are generally allowed so little sag.

ROBERT SABINE.

ABSTRACTS AND EXTRACTS.

ON ACCIDENTS TO SUBMARINE CABLES.

When a deep-sea cable was long ago talked of as possible to be laid across the Atlantic, there were many wiseacres who shook their heads, and prophesied short life to any submarine cable from the voracity of the inhabitants of the deep sea. These opinions found little faith, and were laughed at. Cables have been laid, and for some time the nearest danger was that which happened to the Atlantic cable by the close approach of a large whale whilst paying-out was proceeding. Time has gone on, and the wiseacres were after all not so far wrong, for we have now on record several instances of damages to submarine cables by the inhabitants of the sea.

The first appearance of any damage done to a submarine cable appeared to the Levant cable, laid by Mr. Newall, who speaks of the destruction of the hemp by a species of "teredo." Mr. Siemens speaks to the same effect, and says, "This cable, which was laid in 1858, and taken up again last summer (1859), was found to be beset by another enemy in the shape of millions of small shell-fish or snails, accompanied by small worms, which had completely destroyed the unsheathed hemp, and eaten some circular holes in the gutta percha." Professor Huxley wrote as the result of his examination of these shells: "The specimens you sent me remove all doubt as to the nature of the mischief-maker in the cable. It is a bivalve shell-fish, the xylophaga, closely allied to the ship-worm (teredo), but distinguished from it, among other peculiarities, by not lining its burrow with shelly matter. The zylophaga turns beautifully cylindrical burrows, always against the grain, in wood; and I have no doubt it perforated the hempen coating of the cable in the same way. On meeting the gutta percha it seemed not to have liked it, and to have turned aside, thus giving rise to the elongated grooves which we see. Nothing is known, so far as I am aware, of the range in depth of zylophaga, so that I cannot answer your enquiry as to whether it is probable that cables immersed in 600 to 2,000 fathoms of water would be attacked or not."

In 1860, several portions of cable covered with hemp and steel wire were picked up in the Mediterranean off Minorca; these were found in places, and up to deep water, very much attacked by xylophaga. The hemp between the steel wires being eaten away into holes with the regularity and spacing of those in a cribbage-board. As in previous cases the gutta percha was penetrated to various depths, but not more than the size of the shell-fish. It was generally considered that the xylophaga did not penetrate, owing to its dislike

to gutta percha; but some persons at the time, thought there was a great deal of doubt about the point, for there was no sign amongst the great length of cable so damaged, of any dislike, the main sign being that there had been *no time* for further penetration.

That the xylophaga does penetrate gutta percha to an injurious extent was found to be the case with some experimental wire laid in Kurrachee harbour; this core was subsequently found to be pierced in numerous places, to such an extent as to destroy the insulation. No such severe case has elsewhere been met with, although xylophaga are now met with in almost every sea and every point where cables have been laid. In the Norwegian cable, we have seen specimens which exhibit more than usual penetration.

It may be generally assumed that a core with a solid sheathing of iron wire, so long as that sheathing remains, is safe against the attacks of these destroyers, but that a core cannot be considered safe so soon as an opening presents itself, for, up to the present time, it is impossible to say where they do not exist. In the recent operations connected with the attempt to repair the 1865 Atlantic cable, a portion of the 1858, or original cable, was brought to the surface. Where the wire sheathing had disappeared it was found that the gutta percha had been attacked; "for where the core has been bared there are distinct marks of worms, such as one sees in very old hard timber, or in the rich calf binding of old folios in a library."

We have several reports at different times of cables being attacked by inhabitants of the sea. The Cuba and Florida cable was once damaged by the bite of some large fish, and a similar accident happened to the China cable. In the Malta-Alexandria cable, a piece of core from which the sheathing had been worn, was found to be penetrated by being bitten by a shark, pieces of the teeth being left in the gutta percha, indicating unquestionably the cause.

Some details of a fault which happened some time since to the Singapore cable are interesting. This cable was laid in December, 1870, and the first stoppage occurred in the following March, and was found to be 200 miles from Singapore. The cable was pierced, and pieces of bone were found crushed in the middle of the hole. This specimen was examined for a long time by Mr. Frank Buckland, and it was remarked that the damaged place was caused by piercing, and not by being bitten by the force of a jaw. He accidentally found in his collection a saw-fish, from which he detached the saw, and found that he could make in the cable an incision similar to that which already existed. Mr. Buckland stated that the saw-fish have the habit of penetrating the bottom of the sea in search of food, and giving a backward and forward motion to the species of lance with which they are armed. The extremity of the saw of one of these fishes would become entangled in the exterior wires of the cable; a further brisk movement of the fish would make it penetrate; and, finally, violent struggles would break the saw, after having pierced the cable through and through, as seen in the damaged piece.

Details of a most extraordinary break down to the Persian Gulf Cable came to us from Mr. Izaak Walton, the superintendent of the Persian Gulf telegraphs. He relates that "the cable between Kurrachee and Gwadur (a distance of about 300 miles) suddenly failed on the evening of the 4th instant. The telegraph steamer, Amber Witch, under the command of Captain Bishop, with the electrical and engineering staff under Mr. Henry Mance, proceeded on the following day to repair the damage, which, by tests taken at either end, appeared to be 118 miles from Kurrachee. The Amber Witch arrived on the ground at 2 p.m. on the 6th, a heavy sea and thick fog prevailing at the time, but the cable was successfully grappled within a quarter of a mile of the fault.

"The soundings at the fault were very irregular, with overfalls from 30 to 70 fathoms. On winding in the cable unusual resistance was experienced, as if it were foul of rocks, but, after persevering for some time, the body of an immense whale, entangled in the cable, was brought to the surface, where it was found to be firmly secured by two and a half turns of the cable immediately above the tail. Sharks and other fish had partially eaten the body, which was rapidly decomposing, the jaws falling away on reaching the surface. The tail, which measured fully 12 ft. across, was perfect, and covered with barnacles at the extremities.

"Apparently the whale was, at the time of entanglement, using the cable to free itself from parasites, such as barnacles, which annoy them very much, and the cable hanging in a deep loop over a submarine precipice, he probably, with a flip of his tail, twisted it round him, and then came to an untimely end."

This is, without exception, the most extraordinary accident that has happened to any submarine cable that has come within our knowledge, although many strange accidents have arisen. In one case the cable across the river Yar, in the Isle of Wight, was broken by a bullock, which, falling overboard, got entangled in the cable, finally breaking it.

The causes of interruption we have alluded to are impossible to provide against, with the exception of the attacks of xylophaga, and it is to be trusted that they will always continue to be of very rare occurrence. The use of silica in the compound now generally applied outside all cables is hoped to be a preventative against the insiduous attacks of the persevering shell-fish. Enough, however, has been said to show that there are strange elements in the life of a submarine cable.—*Engineering*.

INDUCED CURRENTS AND DERIVED CIRCUITS.

By John Trowbridge.

The expression for the intensity of an induced current, deduced by Neumann and Sir William Thomson, is as follows:—$i = \frac{1}{k} \frac{dU}{dt}$, in which k is a co-efficient depending upon the resistance of the complete wire in the secondary circuit,

and U is a certain "force-function" which depends solely upon the form and position of the wire at any instant, and on the magnetism of the influencing body. The expression, in general language, is as follows:—

"When a current is induced in a closed wire by a magnet in relative motion, the intensity of the current produced is proportional to the actual rate of variation of the 'forcef-unction' by the differential co-efficients of which the mutual action between the magnet and the wire would be represented if the intensity of the current in the wire were unity."

This investigation was undertaken to ascertain if the laws of derived circuits apply to the currents of induction, which are represented by equations, of which the above is a type. A reflecting galvanometer of large resistance was included in the secondary circuit, and connected by copper wires of very small resistance with the coil in which the secondary currents were produced: the resistance of these wires was infinitesimal in comparison with the resistance of the galvanometer. The galvanometer was then shunted. The first two columns of the following Table show that, with an inappreciable resistance outside of the galvanometer coils, the shunts made no difference in the deflection of the galvanometer-needle when the shunts were not less than three ohms. Below this the current divided. The resistance of the galvanometer was 5880 ohms; and the last numbers in the second and third columns show that an equal impulse was transmitted through both the shunt and the

Exterior Resistances, in ohms.	Shunts, in ohms.	Deflections.	Exterior Resistances, in ohms.	Deflections.
0	3	210	10	210
0	4	210	20	210
0	5	210	30	210
0	6	210	40	210
0	5880	210	100	190

galvanometer; for no reason can be assigned why it should take one course in preference to the other. Two galvanometers, therefore, of the same resistance, one forming the shunt to the other, will give the same deflection, which is equal to that given by the undivided circuit.

Resistances were then introduced into the circuit exterior to the galvanometer-coils, and a shunt of 588 ohms was used.

The fifth column shows that no effect was produced by the shunt until the exterior resistance was appreciable in comparison with that of the galvanometer.

The following Table exhibits the effect of resistances which were appreciable in comparison with the galvanometer resistance. The same shunt of 588 ohms was used. The second column is calculated on the assumption that

$i=\frac{dU}{dt}$ (where k^1 is a co-efficient) is equivalent to $i_1=\frac{E}{R}$, and that the laws of Kirchhoff hold. The third column is obtained from the experimental data. The fourth and fifth columns are also calculated on the assumption that $i=\frac{E}{R}$ =tangent of the deflection. Columns second and third are expressed in arbitrary scale divisions.

Exterior Resistances in ohms.	Calculated value of i.	Experimental value of i.	Ratio of Intensities.	Ratio of Tangents.
1500	1242	1375		
2000	1033	1055	1·06	1·03
2600	954	990	1·06	1·04
3000	767	825	1·06	1·04
3500	673	770	1·11	1·05
4000	613	660	1·05	1·03
4500	558	649	1·05	1·07
5000	514	550	1·04	1·03

It will be seen by comparison that, with large resistances exterior to the galvanometer resistance, and appreciable in connexion with it, the laws of the division of currents practically hold, and as the exterior resistance approaches that of the galvanometer the coincidence with the laws is more marked.

From the above it appears that, under certain conditions, an induced current does not divide according so the laws of divided circuits, but approximates to these laws when there is a resistance exterior to the galvanometer which is appreciable in comparison with that of the galvanometer.--Silliman's *American Journal*, May, 1873.

ELECTRICAL FIGURES ON CONDUCTORS.

By H. Schneebeli.

M. Schneebeli has investigated the conditions on which depend the dimensions of Kundt's electrical figures, which result from the adhesion of a fine insulating powder upon a metallic conductor from which a discharge has just issued.* In his experiments, the discharge of a Leyden jar took place between a horizontal metal plate, sprinkled with Lycopodium-powder (for the production of the electrical figures), and an electrode in the form of a knob, cone, or point surmounting it. The author has found, like M. Kundt, that, *cæteris paribus*,

* Kundt's figures are produced with great neatness on the positive electrode, but are only obtained with difficulty upon a plate serving as the negative electrode. That physicist found that the diameter of the figures increases with the length of the discharge, and in proportion as the diameter of the opposite electrode to the plate diminishes. (*Archives des Sciences*, 1869, vol. xxxv. p. 212.)

the diameter of the figure increases with the distance of the electrode from the plate, but not in a constant ratio; the line which represents the ratios is not straight, but an undulated curve. Also the size of the figure augments with the quantity of electricity which produces it.

When the electrode is composed of a certain number of points, a circular figure is formed beneath each of them. If a small disk of glass is interposed in the path of the discharge, there is produced on the plate a space void of powder and having exactly the shape of the disk.

With electrodes of a conical form with an angle of 30° or of 60°, or in the shape of a tapering needle, M. Schneebeli ascertained that the electrical figure is as much larger as the angle at the summit of the cone is smaller.

Lastly, the diameter of the figure is greater when the discharge is effected in a rarified gas than at the ordinary pressure.—(*Archives des Sciences Physiques et Naturelles*, vol. xlvi., p. 269.)—*Philosophical Magazine*.

ON THE EFFECTS OF MAGNETIZATION IN CHANGING THE DIMENSIONS OF IRON BARS.

By Professor Mager.

The apparatus used was the same as that employed by Dr. Tyndall in his experiments on "the comparison of magnetic and dia-magnetic phenomena." It consisted of two vertical brass rods firmly cemented into a block of stone. Between these rods, securely fixed into the stone, were placed the rods of iron whose elongation he desired to measure. On the vertical rods slid a transverse bar of brass carrying "a vertical rod of brass which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point passes against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates." The angular deflections of the mirror were determined by the method of Poggendorff—that is, by viewing in a telescope the divisions of a fixed scale reflected from the mirror.

Various alterations which were suggested by the experiments were carried out by Mr. Becker, and the longer I worked with it the more mastery I obtained over it; but I did not work with it sufficiently long to perfect its arrangement. Some of the results, however, may be stated here:—

	Figure of scale.
Bar unmagnetized	577
Bar magnetized	470
Bar unmagnetized	517

Here the magnetization of the bar produced an elongation expressed by 107 divisions of the scale, while the interruption of the circuit produced only a shrinking of 47 divisions. There was a tendancy on the part of the bar or of the mirror to persist in the condition superinduced by the magnetism. The passing of a cab in this instance caused the scale to move from 517 to 534; that is, it made the shrinking 64 instead of 47. Tapping the bar produced the same effect.

The bar employed here was a wrought-iron square core, 1·2 inch a side, and 2 feet long.

The following Tables will sufficiently illustrate the performance of the instrument in its present condition. In each case are given the figures observed before closing, after closing, and after interrupting the circuit. Attached to each Table also are the lengthening produced by magnetizing and the shortening consequent on the interruption of the circuit:—

Circuit.	Scale. 10 cells.		Circuit.	Scale. 20 cells.	
Open	647		Open	653	
Closed	516	131 elongation.	Closed	465	188 elongation.
Broken	581	65 return.	Broken	579	114 return.
Open	637		Open	638	
Closed	509	128 elongation.	Closed	452	186 elongation.
Broken	579	70 return.	Broken	568	116 return.
Open	632		Open	632	
Closed	491	141 elongation.	Closed	472	160 elongation.
Broken	568	77 return.	Broken	561	89 return.

These constitute but a small fraction of the numbers of experiments actually made. There are very decided indications that the amount of elongation depends on the molecular condition of the bar. For example, a bar taken from a mass used in the manufacture of a great gun at the Mersey Iron Works, suffered changes on magnetization and demagnetization considerably less than those recorded here —*Philosophical Magazine.*

ON A METHOD OF MEASURING INDUCED CURRENTS.

By F. H. Bigelow.

If a Wheatstone's bridge be formed, in which the secondary coil of the inductorium is the resistance R_1 to be measured, the relation between the resistances will be expressed by the proportion $R_1 : R_2 \quad R_3 : R_4$, where R_3 and R_4 are in a fixed ratio. By passing a current from an independent battery through the primary coil, the strength of the current in R_1 will be increased or

diminished on the breaking or making of the inducing current. Therefore, to preserve equilibrium in the bridge, R_1 must be changed, and we shall have $R_1 \pm C : R_2 + x = R_3 : R_4$, in which x is a resistance equivalent to the effect of the induced current. Let $R_1 \pm C$ be found by trial such that the addition or subtraction of the induced current will produce equilibrium in the bridge—that is, will be such as to bring the deflection of the galvanometer to the zero of the scale. We shall then have the strength of the induced currents expressed as resistances. One advantage of this method is this—that the readings are always reduced to the same point, the zero of the scale. This method also has a wider range than that of merely taking the swing of the galvanometer-needle; for currents which would throw the spot of light in a reflecting galvanometer off the scale can be readily kept on the scale in this method by merely altering the ratio of R_3 to R_4 in the Wheatstone's Bridge. Since the shunting of induced currents is accompanied with difficulties, this method is especially advantageous. When the bridge was set up so that the smallest variation in the resistance of the branch, containing the inductorium, gave the greatest variation in the current going through the galvanometer; namely, when $\frac{dS_0}{dR_3}=0$, S_0 being the current through the galvanometer, and the resulting value of R_3 being $R_3 = \sqrt{\frac{GR_1(2B+R_1)}{G+2R_1}}$, in which G is the resistance of the galvanometer, B that of the circuit exterior to the Wheatstone's bridge, it was found that the induced currents could be measured to one hundred-thousandth of an ohm.

The following Table contains a comparison of the induced currents produced by making and breaking the circuit. The first two columns contain the variation in ohms of the variable resistance of the bridge; the third and fourth columns give the strength of the induced currents on making and breaking expressed in the ohms.

Change in the resistance on breaking.	Change in the resistance on making.	Strength of Induced Current on breaking.	Strength of Induced Current on making.
650	600	·00325	·00300
700	680	·00350	·00340
720	720	·00360	·00360
720	750	·00360	·00375
700	700	·00350	·00350
850	850	·00425	·00425

Care should be taken to send the induced currents to be compared in the same direction by means of a pole-changer. It will be seen from the above Table that the equality of the currents on making and breaking can readily be proved by this method.—Silliman's *American Journal*, May, 1873.

WESTERN UNION TELEGRAPH CO.

REPORT OF THE PRESIDENT.

To the Stockholders of the Western Union Telegraph Company:

I have the honor to submit the following report of the operations of the Company for the fiscal year ending June 30th, 1873, and of the condition of its affairs on that day. The capital stock of the Company at the close of business on that day was $41,073,410, of which there was outstanding $33,778,175, and owned by the Company, $7,295,235. The debt of the Company on that day was $6,038,410, of which there was due October 1st, 1873, for American Telegraph Company bonds, $89,500, in November, 1875, for Western Union currency bonds, $4,448,900; in 1902, for Western Union gold bonds, $1,500,000.

The gross receipts of the Company for the year, from all sources, were $9,333,018·51; the gross expenses, $6,575,055·82; and the net earnings, $2,757,962·69.

This amount has been applied as follows:—

Construction and purchase of new lines	$1,242,205 97
Interest on bonds	317,793 00
Real estate	62,214 40
Purchase of sundry stocks of Telegraph Companies in Western Union Company's system	41,576 69
Paid for $50,000 Western Electric Manufacturing Company's stock ...	39,000 00
Patents (on account of Stearns' Duplex)	19,258 00
Miscellaneous	402 50
Total	$1,722,450 56
The balance has been carried to the credit of income account	1,035,512 13
Total	$2,757,962 69

On the 1st day of July, 1872, the Company operated 62,032 miles of line, 137,190 miles of wire, and 5,237 offices. At the close of the year ending June 30th, 1873, it operated 65,757 miles of line, 154,471 miles of wire, and 5,740 offices. The increase during the year has been 3,725 miles of line, or 6 per cent.; 17,281 miles of wire, or 12.5 per cent., and 503 offices, or 9.5 per cent. For the construction, re-construction and repair of lines, there were used 141,498 poles, 261,715 cross-arms, 1,033,270 insulators, and 1,100,265 pins and brackets. There were in the employ of the Company, on the 30th of June, 1873, 9,190 persons. Of these, four were general superintendents of divisions, 29 superintendents of districts, 5,514 managers and operators, 1,830 messengers, 661 clerks, 589 foremen of construction and repair-men, 116 mechanics in factories, 84 battery-men, and 275 miscellaneous. There were in use on the lines of the Company at that date, 6,350 sets of instruments for

reading by sound, 1,878 recording instruments, 8,601 relay magnets, 8,905 transmitting keys, 227 repeaters, 15 printing instruments, 94 sets of duplex instruments, 2,662 switch boards, 3,472 cut-offs, 3,029 lightning arrestors, 21,777 cups of main battery, and 11,717 cups of local battery. The cost of new instruments and apparatus supplied during the year was $140,877.52, and of battery material and supplies, $146,793.68.

The number of messages transmitted during the year ending June 30, 1872, were 12,444,499, and for the year ending June 30, 1873, 14,456,832, being an increase of 2,012,333, or 16.1 per cent. This includes press reports reduced to messages on the basis of 30 words to each message. The average toll collected upon each message for the last year was 61 cents, the average cost of transmission 42 cents, and the average profit per message 19 cents.

REPAIRS AND RE-CONSTRUCTION.

During the past year, $706,789.58 were expended for repairs of line, and $632,753 for re-construction, making a total expenditure for maintenance of line of $1,339,542.58, an increase over the previous year of $409,537.41, or 44 per cent. The increase during the past year in the cost of maintenance of line is mainly for re-construction, the cost of which was $354,213.28 in excess of the previous year. This increase is principally confined to the Eastern and Western States, and much of it is of an exceptional character, and I hope not likely to recur. Severe storms have prevailed, particularly in January and March, causing great injury to the lines and necessitating large outlays for repairs. Great expense has also been incurred in the removal of lines to accommodate railroad extensions, additions of tracks and sidings, and the removal and erection of buildings, principally on the Pennsylvania Central, New York Central, and Lake Shore railroads; and, to a considerable extent, on many other railroads in the Eastern and Western States. The annual cost of maintenance of the plant for the past five years has been as follows:—Cost of maintenance per mile of line—1869, $14.50; 1870, $15.30; 1871, $15; 1872, $16.59; 1873, 20.37. Cost of maintenance per mile of wire—1869, $7.23; 1870, $7.72; 1871, $7.23; 1872, $7.67; 1873, $8.66. The cost of re-construction was heavier than usual in the Eastern States, on account of the large number of poles which required renewal during the past year.

The maintenance of the plant in the best possible state of efficiency is a necessity which will be readily appreciated, as the capacity of the wires for transmission of messages, and consequently for earning money, depends upon their condition for work in all weather. While every effort has been made to keep the cost of maintenance down to the lowest practicable limit, the average cost per mile has not materially changed during the past five years, and it is not likely that the annual expense will be materially less per mile for the next few years.

Great pains have been taken, during the past seven years, to secure a better and more durable kind of timber for the poles than was formerly used, and it is hoped that the cost of reconstruction will hereafter be somewhat decreased.

In Europe a cheaper class of timber is employed for poles, which are treated with sulphate of copper or creosote to make them more enduring, but investigation shows that the duration of the treated poles in Europe is no greater than that of our best natural timber. The lines which we are now reconstructing were built before the consolidation, and are inferior to those which we are now building, and when reconstructed they are better than when originally built.

It has been the policy of the Company, under its present management, in the direction of repairs and reconstruction, not merely to effect such repairs as would enable the lines to be worked, but to put them in better condition than they had been in before, keeping always in view the inevitable growth of the business, and the constant necessity for providing additional facilities. To illustrate: A line of poles bearing three wires require renewal; they are replaced by poles capable of bearing a dozen wires, and which will last twice as long as the old ones. If only the three wires are placed upon these poles, the whole outlay is charged to profit and loss, although the value of the renewed line is much greater than that of the one replaced when the latter was new. As additional wires are put up on such poles, the cost, merely, of such additions is charged to construction. Under this policy the whole property of the Company is improved every year more than that year's portion of natural decay; so that the apprehension sometimes expressed, of what appears to be the natural result of annual deterioration of the Company's property is groundless. The average condition of the whole property is better at the end of every year than at the beginning.

MONEY TRANSFER SERVICE.

On the 1st of July the transfer system had been in operation 21 months, with very satisfactory results. The increase of the business has been greatly beyond the expectation of its results when established. The revenue of the Company from this source for the first nine months was \$8,936; for the same nine months, one year later, it was \$45,811, an increase of more than five-fold. The revenue from this service, during the past year, was \$58,000; the number of transfers 20,000; the amount transferred, \$1,602,000. The average amount of each transfer was about \$80. Ninety-two new money order offices have been opened during the year. The service has increased in promptitude and efficiency, and measures are now in progress which, it is expected, will still further improve it. Judging of the future by the past, it is not easy to estimate the growth of this business, or to appreciate the results which it may ultimately reach. If the same rate of increase is maintained during the present year that has been developed during the past twelve months, our monthly revenue from transfer will be over \$8,000, or at the rate of \$100,000 per annum. It is a gratifying and healthy feature of the business, that while the number of transfers is increasing rapidly, the average amount transferred is

diminishing; for, as the average amount of transfers grows smaller, the risk of loss diminishes, and the profits of the business are proportionately greater. Experience seems to prove that our transfer system is the best that can be devised. The alterations in the details that have been found advisable since its inauguration have been few and unimportant, while an enlarged knowledge of the business, and familiarity with its working, seems to indicate the necessity only of a rigid adherence to all the regulations established in order to make its operations a complete success.

FREE MESSAGES.

The free messages, so classified in our accounts, transmitted by the Company during the past year, amount, at regular toll, to $766,000. Of this sum, $565,000, or 72 per cent., was performed for railroad and other transportation companies, all of which render us similar service in return. Without this service our lines could not be kept up so cheaply, nor our business so well conducted. Such messages are not free business in the proper sense of the term. They are sent under contracts which stipulate to give the Company an equivalent in right of way, transportation of men and material, and labor to maintain and operate our lines. While the contracts vary in terms they are in the aggregate very favorable to this Company. Of the balance of the free service, $83,000 worth is performed for the directors, officers and operators of the Company; $22,000 on account of rent; $18,000 for connecting telegraph companies; $58,000 for complimentary, and $20,000 for miscellaneous messages. The $83,000 of Western Union business includes the telegraphic correspondence of all the officers and agents of the Company. It has been the constant effort of the management to curtail this business, and the figures now reached are believed to be low and reasonable. The rent account explains itself. The items thus charged are generally limited to a specific sum per month, and the total business is constantly and carefully scrutinized. Instead of paying in cash we pay in telegraphing, and the telegraphing, as a rule, pays more rent than the same amount in money. The items of railroad transportation, telegraph, and rent messages, amount in the aggregate to nearly $700,000, or 90 per cent. of the whole free business.

THE TARIFFS.

During the past year two important modifications have been made in the tariffs. The first change was the abrogation of all Western Union rates above $2·50, which took effect February 1st. The second important change was the extension of the square rate over the territory west of Omaha and east of Utah, and the equalization of the rates in the Eastern, Central and Southern States, which took effect July 1st, 1873. The rates previous to July, 1873, were about 23 per cent. higher in the Western, and 40 per cent. higher in the Southern than in the Eastern States. This inequality was the subject of complaint in the Western and Southern States, and formed one of the strongest arguments in favor of the extension of competing lines and of govern-

mental interference. Another serious objection to these unequal rates was that the special rates between competing points were so low in comparison, that messages for distant points in the West or South could be sent to certain central points over our lines at special rates, and then be forwarded at local rates to their destination, at a considerable reduction from the through rate. For example: The square rate to offices in the vicinity of Chicago was $2. The special rate between New York and Chicago was $1, and as the local rate from Chicago was only 40 cents, our customers on the opposition lines could send messages for such points to Chicago, and deliver them by our own lines, at a reduction of 50 to 60 cents from our through rates. This condition of things existed throughout the South and West and to some extent in the East, and could only be remedied by an equalization of the rates, such as has been recently inaugurated.

EARNINGS IN SEVEN YEARS.

The following statement shows the net earnings of the Company since the consolidation, in 1866, and the disposition which has been made thereof:

The surplus of Income Account, July 1, 1866, was	$275,357 24
The net profits for seven years, from July 1, 1866, to June 30, 1873, have been	20,312,618 81
Making an aggregate, June 30, 1873, of	$20,587,976 05
Of this sum there has been—	
Distributed in dividends to stockholders	4,857,239 34
Disbursed for interest on the Company's bonds	2,216,194 98
The balance	$13,514,541 73
is represented as follows:	
Construction of new lines and the erection of additional wires	$4,405,180 44
Purchase of telegraph lines and of the stock of Companies controlled by the Western Union Company, on which interest or dividends are paid as rental	695,428 95
Western Union stock (72,952 shares)	4,054,483 07
Gold and Stock Telegraph Company's stock (47,710 shares)	1,173,509 00
International Ocean Telegraph Company's stock (10,384 shares	961,556 42
Pacific and Atlantic Telegraph Company's stock (49,917 shares)	510,274 50
Anglo-American Telegraph Company's stock (£1,308 0s. 0d.)	10,000 00
Western Electric Mfg. Co.'s stock (500 shares)	39,000 00
Western Union Bonds, redeemed and cancelled	974,075 00
Western Union Broadway and Dey street Mortgage Sinking Fund ..	30,000 00
Real Estate (exclusive of Broadway and Dey street property)	318,263 14
Patent— The Page and Duplex Telegraph	73, 758 00
Cable steamer	12,665 19
Western Union Bonds not cancelled ($7,500)..	6,750 00
Fraction of share (old issue) redeemed and cancelled	42 50
Total	$13,264,986 21
Leaving an excess of	$249.555 52

which excess is applicable on account of Sinking Fund appropriations not yet used for redemption of bonds.

All the above items are entered at the actual cost to the Company, in cash, except the $1,173,509 in stock of the Gold and Stock Telegraph Company which is stated at par, and we should not be willing to sell it at that rate Many of the other items are worth much more than cost.

THE INTERNATIONAL OCEAN TELEGRAPH COMPANY.

During the past year, the Western Union Company have acquired control of the International Ocean Telegraph Company, whose lines extend from Lake City, Fla., to Havana, Cuba. The capital stock of this Company is $1,500,000, of which $850,000 is in common, and $650,000 preferred stock. The Western Union Company purchased of the common stock, $538,500, and of the preferred stock, $499,900, making a total of $1,038,400 at the par value. The actual cost to us was $961,556.42. The International Ocean Company possess a fifty years' exclusive grant from the Spanish Government to lay and operate cables between Cuba and the United States, and a fifteen years' exclusive grant from the Government of the United States to lay and operate cables between the coast of Florida and the West India Islands. When the controlling interest in the stock of this Company was purchased, communication by cable between Florida and Havana was interrupted. In April last one of the cables between Key West and Havana was repaired, and a new one also laid down, and both are now in good working order. The lines of the International Ocean Company are now doing a good and profitable business with Cuba, Porto Rico, and Jamaica, and when the cables connecting Jamaica with the other West India Islands, and to Central and South America are completed, which, it is confidently expected, will be in a short time, the traffic upon these lines, which constitute the only outlet for the telegraphic correspondence of these countries with the United States and Europe, will become very large and important. The net profit of the International Ocean Company is now at the rate of about 200,000 per annum.

PACIFIC AND ATLANTIC.

We have also acquired during the last year a majority of the stock of the Pacific and Atlantic Telegraph Company, and negotiations have been in progress for some time with a view to leasing their lines. The capital of that Company is $2,000,000. Their lines extend from New York via Philadelphia and Baltimore to Pittsburg, Columbus, Indianapolis, St. Louis and St. Paul, branching at Dubuque to Chicago, and from Chicago to Cincinnati, Louisville, Nashville, Memphis and New Orleans. They own about 5,000 miles of line and 10,000 miles of wire, and the gross receipts last year were about $500,000 It is believed that an arrangement will be made, satisfactory to the holders of the balance of the stock, by which the lines will be turned over to the Western Union Company at an early day.

PNEUMATIC TUBES AND SUBTERRANEAN LINES.

In February last the Executive Committee authorised me to send Mr. George B. Prescott, the Electrician of the Company, and General Thomas T. Eckert, the General Superintendent of the Eastern Division, to Europe, for the purpose of making a careful inspection of the system of pneumatic tubes and subterranean telegraph lines now in operation in the principal cities, and also to examine the working of the telegraph lines generally. They found the most complete system of pneumatic tubes and subterranean lines in operation in London, although both systems were extensively employed in Paris, Berlin, and other cities. In London about 13 miles of pneumatic tubing have been laid down, at a cost of £16,000, and about 10,000 messages per day are sent through them. The average time occupied in the transmission of messages through the pneumatic tubes is about two minutes, and the number of messages transmitted in each carrier is from eight to twenty, according to the size of the tubes. There are thirteen stations in the City of London connected by pneumatic tubes; and it is the opinion of Mr. Scudamore, the Manager of the English telegraphs, that it would be impossible to properly transmit the large number of messages between these offices in any other way than by pneumatic tubes. The system of underground lines in England embraces 3,000 miles of wires and nearly 100 miles of iron piping. There, are upon an average, 60 wires in each pipe, and the average cost per mile of wire is £23. While the first cost of underground lines is considerably greater than for those on poles, the interruption in the working of them and the cost of maintenance are claimed to be less, and the system is favourably regarded for the large cities. Messrs. Prescott and Eckert obtained minute and accurate information as to the best methods of constructing pneumatic tubes and underground lines, as well as much other valuable information in relation to the working of the telegraphs generally, which will be found of great importance in the future operation of our lines.

When our new building on the corner of Dey Street and Broadway is completed, it is proposed to connect it by pneumatic tubes with the Stock and Produce Exchanges, and other important branch offices in the city. It is also contemplated to lay down subterranean lines in the lower part of the city, with the view of testing their efficieney and economy as compared with the present mode of construction.

STEARNS' DUPLEX TELEGRAPH.

The purchase of the patents for the United States and Canada of this most important and valuable of all the improvements which have been made since the Morse telegraph was first established, was mentioned in my last Annual Report. During the past year we have largely increased the number of Duplex instruments and apparatus, by means of which messages can be

transmitted in opposite directions upon a single wire at the same time. We have now in operation over 100 sets, and it is the intention to have all our principal stations completely equipped with them as soon as they can be supplied from the factory. We are now operating more than 150,000 miles of wire, and during the past two years have been extending at the rate of nearly 20,000 miles of wire per annum. The Duplex apparatus is capable of doubling the capacity of these wires at a comparatively small cost. The value of this increase of facilities can be approximately ascertained by estimating the saving in the investment for wire and the annual saving in repairs and maintenance of additional wires. But the great value of the Duplex does not consist in the saving in the investment in wires and the cost of repairs and maintenance, but in its ability to double the capacity of a wire when we have but one, and when no amount of money previously invested in wires, or even possible to be expended in repairs, can provide another. Sometimes a storm or conflagration causes the interruption of all the wires on an important route, at a time when every wire is needed to keep business moving. Communication is restored upon one wire at a time, and it is then that the great value of the Duplex becomes apparent. With it the moment one wire is restored we have two, and when we have a second we have four, and so on.

During the extraordinary excitement of the past three weeks, our wires between the principal cities have been taxed to their utmost capacity, and if we had double the number on some routes they would have been insufficient. Every set of Duplex has been brought into requisition, and only our inability to procure them has prevented the use of a larger number. A minor consideration, and yet important one, is the fact that the Duplex can be substituted for, and its use does not involve the duplication of the ordinary apparatus. It works equally well double or single. As our operators acquire experience in its use, the difficulties which attended it at first wholly disappear. They now adjust with such facility that the change from single to double is made in less than a minute, and I confidently expect that, by the time the necessary instruments can be manufactured, they will be largely introduced on railroad and other way wires.

GOLD AND STOCK TELEGRAPH COMPANY.

The Western Union Company own 47,710 shares, of the par value of $25 per share, of the stock of the Gold and Stock Telegraph Company, out of the capital of 100,000 shares. The Gold and Stock Telegraph Company transmits its reports over the Western Union lines, and during the past year paid us in tolls $77,995, and one dividend of three per cent. amounting to $35,970. Within the last two years the Gold and Stock Telegraph Company have added to their property, in instruments, poles, wires, fixtures, &c., about $400,000. The gross receipts of that Company in 1871 were $236,215; in 1872, $534,780, and in 1873, $623,900. They had earning revenue:

In 1869	572 instruments	In 1872	1,594 instruments.
In 1870	741 instruments	In 1873	1,783 instruments.
In 1871	830 instruments		

The Private Line Department in the City of New York has a rental of 49,290 per ann um, and is steadily increasing. The net income of the Company from private lines in other cities is equal to that of this city, and is also increasing steadily.

In order to be prepared for the large increase of business, the expenditure for construction has been very large, and has interfered with the payment of regular dividends, but the Company is now becoming well supplied with lines and equipments, and it is confidently anticipated that regular dividends will be paid hereafter.

THE COMPANY'S FACTORY.

In January last the factory in New Church Street was completed at a cost of $139,772, of which $85,000 was paid for the land and building. The lot is 104 feet on New Church Street, by an average depth of 36 feet. The building is admirably adapted to the manufacture of telegraph apparatus, and is in close proximity to the permanent head quarters of the Company in their new building. The factory is now capable of supplying all the apparatus required by the Company, and its capacity can be more than doubled when required. The operations of the factory for the past six months show a small profit, after deducting the interest on the investment. The apparatus made at this factory greatly excels any other manufactured in this country, and it is the superior quality of the material and workmanship, rather than the saving in the cost of the instruments, which constituted the great inducement for establishing it.

THE NEW BUILDING.

The avails of the $1,500,000 of bonds issued on the mortgage of the real estate at the corner of Broadway and Dey Street amounted to ..		$1,497,883 69
Accrued Interest received ..		15,106 34
Interest on avails unexpended, July 1, 1873 ..		30,075 00
Received for sale of old material ..		1,000 00
Total received up to July 1, 1873 ..		$1,544,065 03
Expenditures for purchase of the real estate at the corner of Broadway and Dey Street, up to July 1, 1873 ..		855,000 00
Interest on bonds ..	$121,012 50	
Taxes, &c., &c. ..	10,869 56	
Interest on Dodge Contract ..	3,320 76	
Commission on sale and expense of preparing bonds ..	14,730 84	
Amount paid to architect and builders ..	220,048 04	
		369,981 70
Total expended to July 1, 1873 ..		$1,224,981 70

The work on the building has not progressed as rapidly as was expected, and it will be impossible to occupy it, as originally contemplated, in the Spring

of 1874. The delay has been occasioned wholly by the inability to procure the granite within the time required. Some changes have been made in the original plan, by which the cost will be somewhat increased. All the work is being done in the most thorough and substantial manner, and the building promises to be, when completed, one of the finest in the city. The total cost for land and building, furnished and equipped with the machinery and apparatus required by the Company, will be about $1,700,000. The basement and cellar will be occupied by the Company. The first, second, and at least one other story, will be offered for rent. It is not expected that the annual cost to the Company in excess of the income from rents will be much, if any, greater in the new building than has been paid for the premises now occupied, while we shall have double the space, and superior accommodation in every particular.

At the date of the consolidation with the American Telegraph Company, July 1st, 1866, the capital of the Western Union Company issued and liable to be issued was, at it now is, $41,073,410. The Company was then in possession of about 75,000 miles of wire, on which were transmitted during the year following about 5,800,000 messages. At that time all the lines on the Pacific Coast, and nearly all in the States of Iowa, Illinois, and in Northern Missouri, although working in connection with ours, were owned and controlled by other companies. Although the Western Union Company owned a line between the Missouri River and Salt Lake City, at which point connection was had with the California and Pacific Coast lines, its business from Chicago to the Missouri River was obliged to pass over the lines of another company. Since that time we have acquired, by lease and purchase, the lines of the Illinois and Mississippi Company which occupied the States of Illinois and Iowa; of the Chicago and Mississippi Company, which occupied Northern Missouri; and of the California State Company, which occupied the territory on the Pacific Coast and west of Salt Lake City.

On the 1st of July, 1873, the Western Union Company controlled and operated more than 150,000 miles of wire, and owned the controlling interest in companies operating over 10,000 miles more.

During this period of seven years, in which the Company's lines and wires have been doubled in extent, and the capital stock outstanding has been reduced by more than $7,000,000, and other property acquired, representing in the aggregate more than $13,000,000, the volume of business has increased from less than 6,000,000 messages in 1866, to about 14,500,000 in 1872. The gross receipts during the same period have grown from $6,500,000 to $9,333,000, but the profits have not increased either in proportion to the volume of business or to the gross receipts, the excess of 1873 over 1866-7 being but about 130,000. A brief statement of the causes which have produced this result seems proper at this time. They are mainly these:—

1. The enhanced cost of labour, of poles, wire, and all material used in constructing, maintaining and operating the lines.

2. The reduction of rates rendered necessary by the action of competing companies between stations on the route of their lines, in the first instance; and later reductions in other sections in order to equalise rates, and thereby remove the inducement for competing lines to extend still further.

No public business of equal importance is so sensitive to competition as that of the telegraph, and with no other can competition be inaugurated by the outlay relatively of so small an amount of capital. Two classes of persons are always interested in promoting telegraphic competition—first, those who organize the Companies, and hope to profit by contracts for construction of the lines, or by stock bonuses and commissions on the sales of stock; and, second, the senders of messages, who are willing to risk the income from a moderate investment if thereby a larger sum can be saved annually from the reduction of telegraph tolls which a competing line is expected to secure.

In 1866, the only telegraph lines in the country competing with the Western Union were those of two companies—one extending from Boston to Washington, and the other from New York to Washington. The latter, let me remark, after sinking its capital of about $750,000, was sold for debts contracted, amounting to $120,000 more, and afterwards leased by the purchaser to the Western Union for six per cent. per annum on that sum. But in 1867 and 1868 several new companies were organized, and the work of constructing competing lines were pushed with much earnestness during the next three years. Parties willing to subscribe liberally to the capital stock of these companies were readily found, some influenced by the promise of ten per cent. annual dividends on the par of shares for which they were asked to pay but forty per cent., and that in quarterly instalments, and others by the argument that, at the reduced rates promised, a handsome profit would result from the saving on their own messages, even if no dividends were paid on the capital. The effect of this competition upon the Western Union Company's business may be readily seen by an inspection of the results of 1870, as compared with 1869. For the latter year the gross receipts were about $7,300,000, and the net profits $2,750,000, while for 1870, although the gross receipts were almost the same, the profits were less than the previous year by more than $500,000, and it was not until 1872 that our profits exceeded those of 1869, and then by only $40,000.

At the same time that competition has been operating to reduce the rates and increase the expenses; we have been called upon at every session of Congress, for the last four years, to defend the policy and management of the Company from charges made by members of both Houses of Congress, by officials of the Government, and in a few instances by the press. The effect of these hostile proceedings against the Company has been to induce the Executive Committee to proceed more rapidly with the work of increasing the facilities and reducing the rates than the actual condition of the Company's affairs may, at first sight, seem to justify. But little reflection

will, however, be necessary in order to satisfy you of the wisdom of their action.

The scale of rates fixed by competition on the most important routes, and between the principal cities, has been applied recently to the whole country east of the Rocky Mountains, so that the inducement to subscribe capital for the extension of competing lines, in order to secure the benefit of competing rates, no longer exists. At the rates now established it is impossible for any competing company to realise profits, and some of them are known to be, and all are believed to be, operating at a loss. As a result, the extension of competing lines has ceased, and it is not believed that capital can be found wherewith to inaugurate new enterprises in any quarter. The time is not distant, therefore, when the Western Union Company will be without a substantial competitor in the conduct of a business which, notwithstanding the enormous growth of the last seven years, is still in its infancy. With the increase of wires already provided and now in progress, the capacity of which the Duplex Apparatus, hereinbefore spoken of, will be able to double at small cost, it is believed that the constantly increasing volume of business, the growth of which will be stimulated by the present low and uniform rates, can be successfully handled with a less annual investment in new construction than has heretofore been necessary; so that, with competition checked and in process of being extinguished, the percentage of expenses may be reduced, and the patience of the stockholders be rewarded at an early day by the resumption of regular dividends.

Respectfully submitted,

Journal of the Telegraph. WILLIAM ORTON, *President.*

ON THE OVERLAND TELEGRAPH.

ON Monday evening the final lecture of the series which has been held in aid of the Building Fund of the Stow Congregational Schoolroom, was delivered by Mr. Charles Todd, C.M.G., Postmaster-General and Superintendent of Telegraphs, on "The Overland Telegraph." His Excellency the Governor presided, and Mrs. and Miss Musgrave, attended by the Private Secretary, were also present. There was a good attendance.

His EXCELLENCY said it seemed almost absurd that he should have to introduce to them Mr. Todd, who was so much better known to them than he had the honor yet to be. In asking Mr. Todd to deliver his promised lecture he felt that that gentleman would say something of so much more interest than anything he could say that he need not trouble them by making any preliminary remarks.

Mr. TODD, C.M.G., then commenced his lecture. After referring to the grand achievements of modern science, he spoke of the gloomy forebodings

which many people held respecting the construction of the Overland Telegraph, which was projected two or three years since. The whole history of telegraphy was a series of triumphs, and no country could progress which had not rapid communication, for rapid communication was the life of a nation. Having briefly sketched the early introduction of the electric telegraph into England and America about 1846, he said it speedily became a valuable adjunct to railways, and an important auxiliary of commerce. Having sketched the rapid extension of the telegraph by land and sea, linking country to country, and continent to continent, he referred to the schemes propounded for connecting Australia with the telegraph systems of the world. The British-Indian, now the Great Eastern Telegraph Company, laid a continuous chain of cables from Falmouth to Lisbon, Gibraltar, Malta, Alexandria, down the Red Sea to Aden and Bombay. The British-Indian Extension Company carried it on from Madras to Penang, and at this stage the British-Australian Telegraph Company was started to connect Singapore with the Australian land telegraphs, and Commander Noel Osborne visited these colonies in the early part of 1870 to complete the necessary negociations. The first proposition came from Messrs. Brett and Carmichael, in a letter addressed to the colonial Governments in 1854. This offer was renewed in 1858, and when it first came before him, the proposal was to lay a cable from Ceylon to the west coast of Australia in two sections connecting at the Cocos, or Keeling Islands. Another scheme was submitted in 1857 by Messrs. Carr and others; but the gentleman whose name was most intimately associated with Anglo-Australian telegraphs, although he was unfortunately unsuccessful, was Mr. Francis Gisborne. Mr. Gisborne's labours extended over 11 years, and he no doubt was instrumental in keeping the question before the public, and ultimately inducing English capitalists to take it up. He felt it due to Mr. Gisborne thus publicly to acknowledge his unremitting and valuable services, as it was his misfortune in advocating the Overland Line to have, to some extent, to oppose his views. He visited the Australian colonies in 1859, with a view of obtaining subsidies from the local Governments for cables 3,700 knots in length, to be laid from the east end of Java to Brisbane, the cost of which was estimated at £800,000, which was increased in 1862 to £1,100,000, on which outlay a subsidy of £50,000 a year was asked. He first wrote officially on the subject in the early part of 1857, but it was not until after the explorations of Mr. A. C. Gregory in 1856, and those of Messrs. Babbage, Warburton, and Stuart in 1858, that he was led to consider the feasibility of carrying a line overland from Port Augusta to Cambridge Gulf. In July, 1859, he wrote a letter to Sir Richard MacDonnell, giving plans and estimates of the line he proposed, and these were subsequently embodied in a despatch from the Governor to the Secretary of State in the October following. His estimate showed that a saving of £400,000 would be effected by carrying out the land line, besides which the land line would open up the unknown interior, and form a settle-

ment on the north coast, whence we might ship horses to India. The whole thing, however, was premature then, and any attempt to connect Australia with the Indian telegraphs would have been a failure. Mr. Stuart's explorations in 1860, 1861, and 1862 fully confirmed the favorable view he took, and on his return from his last successful exploration, in which he reached the north coast near the mouth of the Adelaide, he reported that the line was perfectly practicable, and would pass through a vast extent of country available for settlement. Having adduced proof of telegraphy being the forerunner of railways and subsequent settlement, he referred to the energy with which Queensland had extended her land lines northward till she reached Cardwell in 1870, and Normanton in the early part of 1872. In 1869 several projects were started, two of which were for a western route, one to Ceylon, and the other to Java *via* the North-west Cape. Mr. Fraser also appeared in the field in favor of a line from Java to Normanton; and Mr. Gisbone was hard at work, when the Telegraph Construction Company took it up. The British-Australian Telegraph Company was launched, and undertook to bring the cable to our doors without either subsidy or guarantee. This, while no doubt due in a great measure to rivalry, and a desire to keep others out of the field, was also owing to a timely letter from Mr. R. D. Ross, published in the London *Times*, drawing attention to the large trade between the Australian colonies and Great Britain, and the probability of the telegraph paying a good return on the outlay. Mr. Ross was the first to place these facts clearly before British capitalists, and this came at a most opportune moment. The British-Australian Telegraph Company's scheme comprised a cable from Singapore to Batavia, where they would join on to the land line through Java to Banjoewangie, and from Banjoewangie a cable was to be laid to Port Darwin, and a land line thence to Normanton; but, although the prospectus provided for that, it was by no means certain at that time that the Company would come to Port Darwin at all, as little confidence was felt in the land section, which it was seen would be difficult and expensive for an English Company to construct and maintain, as it would require several intermediate stations; and if that part of their scheme were altered, as no doubt it would have been, there remained no inducement to come to Port Darwin at all, it being out of the way, and the cable would have been laid to Normanton. Fully realising that, on the arrival of Commander Osborn in April 1870, he took the first opportunity of discussing with him his (Mr. Todd's) old project of a land line directly across the continent, and pointed out the great advantages such a line would possess over that of Queensland. He also showed that our line would be much shorter, and that Queensland could easily tap it by an extension from Normanton. Mr. Strangways, who was then Attorney-General, and in whom the overland line had found one of its earlist promoters in connection with Stuart's explorations, then took the matter up very warmly, and on receiving an official report from him, a Bill, authorizing the necessary loan, was drafted

and laid before Parliament, and notwithstanding a ministerial crisis, the Bill was passed by a large majority, and then, perhaps, for the first time he fully realized the vastness of the undertaking he had pledged himself to carry out. He was as sanguine as ever with regard to the practicability of the thing, but the short space of time allotted to him—only 18 months—greatly increased his difficulties. The Bill was passed in June, 1870, and, under the contract, he had to open communication with Port Darwin by the 1st January, 1872. Only a few months before, the duties of Postmaster-General, in addition to the management of the Telegraph Department, had been transferred to him, and then he had to carry a line of telegraph, 1,800 miles in length, through the very centre of Australia, 1,350 miles of which was wholly unsettled by white men —through a country of which he knew positively nothing, except what Stuart had told them of the narrow strip he had traversed. The wire had to be procured from England, the insulators from Berlin, and when the order went home the cruel war had broken out, and serious delay occurred in getting the insulators through Belgium. These, on arrival, as well as provisions for the workmen and a vast quantity of other materials and stores, had to be carted over many hundreds of miles of rough country without roads; some from Port Augusta, others, after reshipment, from Port Darwin. He knew that much of the country, north of Port Augusta, was destitute of serviceable timber, and further acquaintance showed him that he had something like 400 miles of timberless country to bridge over. He then referred to the dispatch of Mr. J. Ross to explore the country as far north as the MacDonnell Ranges, arranging to meet him on his return at Mount Hamilton; also to the various sections which were let under contract to be constructed, the central and Northern Territory portions being carried out by Government parties in charge of competent surveyors, the Northern Territory section being divided between two parties—one working from Port Darwin as a basis, the other to be dispatched to the Roper, to commence somewhere near the Roper. Had that plan been adhered to it was his firm conviction—and he said it with all the experience he now had—the work would have been completed in time, and a vast amount of money would have been saved. He added, however, that everything was done after careful deliberation, that course being adopted which at the time appeared to be the best. The Government parties started from Adelaide in August, 1870. The first pole was planted at Port Darwin about the middle of September, and at Port Augusta on 1st October, 1870. In October, 1870, he went so far north as the Peake to meet Mr. Ross, and make final arrangements for the disposition of his forces. So far all went well, without a hitch. The country in the interior was even better than was anticipated, and there seemed every prospect of the work being completed within the specified time, when their hopes were blighted by the return of Mr. W. W. McMinn in the Gulnare from Port Darwin, who reported that the contractors' expedition in the Northern Territory had collapsed. That was

early in July, 1871. In organising a fresh expedition in the Northern Territory, he strongly urged that the Roper should be the main basis of operations, especially as the poles had been erected for 225 miles from Port Darwin, and the adoption of the Roper would save nearly 250 miles of land carriage. It was, however, decided that less risk would be incurred by landing at Port Darwin, and a large expedition was rapidly fitted out, and placed under the charge of Mr. R. C. Patterson. Mr. Patterson fully concurred with him as to the value of the Roper, and, immediately on his arrival at Port Darwin, he (Mr. Patterson) arranged to send the Government schooner Gulnare round to the Roper with wire and stores, but unfortunately she was wrecked the day after starting, and Mr. Patterson transhipped her cargo into the Bengal, which vessel he chartered to proceed to the Roper on the same errand. The time of year was very unfavorable for working stock immediately after a sea voyage. It was drawing towards the end of the dry season, when all the feed was burnt up and the country was bare. Numbers of the horses and nearly one-third of the bullocks died, and the loads had to be lightened or abandoned on the road before the Katherine was reached, and further on it was necessary to sink wells before the teams could advance with safety; and ere this was accomplished down came the rains, and a monsoon of unusual severity set in almost before the work could be resumed, and stopped all further progress for months. In the midst of these misfortunes the cable fleet arrived at Port Darwin; the shore end was landed, the vessels started to pay out the cable to Banjoewangie, and in November communication was established with London, one of the first items of intelligence being—"South Australian land line not nearly completed." It was under these circumstances that Mr. Patterson telegraphed reporting his losses, and urged that large reinforcements should be sent up at once. The steamers Omeo and Tararua were then chartered, and he (Mr. Todd) proceeded in charge to take any steps he might feel necessary to secure the completion of the work. The Young Australian having been dispatched in advance, he sailed in the Omeo from Port Adelaide on the 4th January, 1872, in charge of a shipload of horses; but the day before he left communication was established with the MacDonnell Ranges, and was gratified by hearing that the central sections were completed, and the work progressing fast north of section E. But for their unexpected misfortunes in the Northern Territory he would have been able to fulfil South Australia's bargain. He likewise received the sad news that Mr. Kraagen—who was to have taken charge of the station at Alice Springs, in the MacDonnell Ranges—had died from want of water. He then alluded to his arrival at the mouth of the Roper, his meeting Mr. Patterson, and to his deciding to proceed in the Omeo up the Roper, after signing an agreement indemnifying Captain Calder from any loss which might accrue to the Omeo in crossing the bar. They only proceeded a few miles when the vessel got stuck on a sand reach, where she remained for several days, and in the meantime, the Young Australian having

arrived, he went in her up to the landing place with some of the cargo, passing the Bengal, where she had been anchored since the middle of December. Soundings being taken, they found there would be plenty of water, varying from three to nine fathoms in depth, but that care would be required to avoid several nasty rocks. The river was between 400 and 500 yards wide at the entrance, and for some 30 miles up, and at the landing, or depot, about 120 yards wide. At some points, particularly at Moleshill, the scenery was most beautiful, and some large swamps were profusely covered with blue lilies, while all around was fine park-like country. The river was navigable up to the depot for vessels drawing ten to eleven feet all the year round, but in the dry season the navigation was difficult and dangerous because of the rocks. Above the landing there were several bars, the first of which, some miles higher up, being known as Leichardt's, which impounded the water in the upper river, forming fine long deep reaches, with high banks thickly lined with melaleuca, corkscrew pines, and fine casuarinus. The river during the wet season is subject to immense freshets, when the waters rise at the landing over 30 feet, and the country is flooded for miles. It did so when he was at Maria Island—14 miles from the mouth. A jetty had been erected at the landing, where the horses were landed. Continuous and heavy rains lasted all through February and March, during which nothing could be done, but directly the fine weather set in and the roads became passable, they loaded upt teams, and Mr. Patterson went on to the working parties, while he went to Port Darwin to complete all the arrangements, and inspect the building there, and have the line between Port Darwin and the Katherine thoroughly overhauled, their enemies, the white ants, having brought it to grief during the wet season and stopped the communication. He arrived at Port Darwin on 8th May, and was very pleased with the place, and greatly admired its fine natural harbour, which was destined, he believed, to become a port of large commerce. The only decent buildings were the Residency and the Telegraph Offices, which stood on elevated ground commanding a fine view or the harbour. He went up to Southport, where he landed a large number of iron poles, and carefully inspected several miles of line, and having completed his task, started on his return to the Roper on May 22nd, arriving on the 31st. He wished, if possible, to return overland from Port Darwin, but no horses could be spared. After staying at the Roper he started with Mr. Patterson for the Daly Waters on his overland journey to Adelaide, on June 13th making a final start, arriving at Daly Waters on the 22nd, where he met Mr. Boucaut. He then referred to the establishment of an estafette, or horse express service between this point and Tennants Creek, and to his having spoken Port Darwin, and sent a message to the agent-General, in London, informing him of the state of the work. On the following day a number of messages came through from London, but before he could get a reply the cable between Port Darwin and Java broke, and communication was not restored for several months. Mile

after mile the communication with Adelaide was maintained on the land line by a field operator, and the gap was then gradually closed, and finally the two ends of the wire were joined on August 22nd, and communication was established right through between Adelaide and Port Darwin—a distance of nearly 2,000 miles. Thus the great work, notwithstanding all disasters and mishaps, was successfully accomplished within two years, and he thought he might with confidence assert that no line passing through a similar extent of uninhabited country, where the materials had to be imported and carted over such long distances, or country representing similar natural obstacles, had been constructed in the same short space of time. When the wires were joined he was at Central Mount Stuart, and in the evening he was inundated with kindly-worded messages of congratulation from friends in all parts of the colonies. He had intended giving some description of the country, but time would not permit. Travelling in Australia was, after all, very monotonous. There was an absence of life except occasionally a few poor wandering blacks, and a general scarceness of scenery that was rather depressing, to which, however, an antidote was found in the bracing south-east breeze and wonderful transparency of the atmosphere, which were most enjoyable and exhilarating. Of course the journey was not all monotonous, for he had plenty to occupy his thoughts, and at every station he was busily employed from morning till night. The stations were all well stocked and supplied with every requirement. On his arrival at Beltana on October 18th, he heard that the cable had that day been picked up by the repairing ship which had signalled the Port Darwin Office, and on the following Monday communication was restored, and Australia for the first time reaped the fruits of South Australia's enterprise. He remarked that he need not enlarge on the advantages to the other colonies of telegraphic communication. The work which they undertook and successfully consummated, though single-handed, had, it was true, proved a costly one—far more costly than they anticipated, but repayment would be speedy. To take one fact—without the telegraph it would have been impossible for South Australia to have disposed of the large surplus produce of last harvest, except at such a sacrifice as would have ruined their farmers. With the telegraph, the wants and prices of all the markets of the world were known to them without delay. And beyond that they possessed the means of securing ships from every quarter, till their ports were crowded with the finest fleet ever seen in South Australian waters, ready to carry away their golden grain to the millions who were eager to consume it. He was assured by merchants, most competent to form an opinion, that the telegraph had realized for the colony at least £150,000, in the advanced price it had enabled us to obtain for our wheat. The telegraph might check unhealthy speculation, but it made commerce safer, tended to equalize prices, put the farmer, merchant, and consumer on a footing of fair equality, and by the more speedy liberation of capital it cheapened all commodities and the necessaries of daily life. The telegraph had already led to the taking up of large

tracts of country in the interior and had done more than anything else to develope and make known the rich resources of the Northern Territory. On a map of the world he had shown all the principal lines of telegraph, and it would serve to show the importance of the line they had constructed, when he told them that it connected them with a telegraphic system whose ramifications extended over 330,000 miles, or about 600,000 miles of wire, besides 33,000 miles of submarine cable. In Great Britain alone, there were 25,000 miles of line and 85,000 miles of wire, through which 15,500,000 messages were transmitted last year, no less than 28,502 messages passing through the London Office in one day (February 26th, 1872), whilst in the whole of 1851 there were only 48,490. In America they had about 77,000 miles of telegraph, or 165,000 miles of wire; and it might be interesting to know that in Australia and New Zealand there were about 20,000 miles of wire, through which nearly 2,000,000 messages were passing yearly. In the first six months of the present year, the value of cable messages transmitted over their Overland Telegraph amounted to no less than £54,000. With such facts as these before them, well might they say, with Mr. Schutz Wilson—

> "Lords of lightning we; by land or wave
> The mystic agent serves us as our slave."

Mr. Todd was frequently applauded, and at the close of his lecture he was very heartily cheered.

Much interest was felt in the fact that the telegraph wires were laid on to the room, and at the termination of the lecture, Mr. Todd sent messages to Alice Springs Station, on the Overland Line, and to Port Darwin, and also to Banjoewangie, Singapore, and Batavia, and answers were returned in a very few minutes. In reply to a question, the Port Darwin Station said—"The Omeo will sail in about a fortnight." Mr. Todd sent the following telegram to Banjoewangie, Singapore, and Batavia:—"Adelaide. I am giving a lecture on Anglo-Australian Telegraph, Governor in the Chair. Parliament met Friday. Change of Ministry. Have you any news? What time is it?" The following were the replies received:—From Mr. Bell, Superintendent of Telegraphs in Java—"Batavia (a distance of 3,700 miles by telegraph)—Mr. Todd, I am perfectly well and hope you are the same. Regret cannot be present to profit by your lecture. No news whatever here. 17 minutes past 7 p.m." From Banjoewangie:—"Fever raging here. Weather hot. Dutch making active preparations renew attack Acheen in December. Reinforced by troopship and commander of expedition from Holland. Time, 7.11 p.m." From Singapore—"7.30 p.m. No news to send. Wish you success. Am quite well; hope you are the same." All present seemed to be delighted with this practical illustration, as Mr. Todd termed it, of the way in which they almost annihilate space.—*Australian Paper.*

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. II. 1873. No. 6.

The Eighteenth Ordinary General Meeting was held on Wednesday, the 12th November, 1873, Mr. CROMWELL F. VARLEY, F.R.S., Member of Council, in the Chair.

The following Paper was read "ON THE QUADRANT ELECTROMETER," by Mr. JOHN MUNRO, C.E.

SECTION I.

REQUIREMENTS OF AN ELECTROMETER.

1°. WITHOUT proper means of measurement no science can prosper and become applied, and as every electric phenomenon is primarily due to differences of the electric potentials established among matter, an electrometer, or instrument for measuring differences of potentials, is a primary necessary to the advance of our knowledge of electricity and our application of it to the Arts. Since the last century various rudimentary electrometers have been contrived for experimental purposes, but these have been more or less crude and imperfect. The modern electrometer, besides answering the uses of scientific research, should with advantage be fitted for the commoner work of telegraphy. It should be adapted to measure alike the agency of the thunderstorm and secular variations in the electromotive force of the most constant galvanic element. Its degree and range of sensibility should therefore be great and readily modified. The Thomson Quadrant Reflecting Electrometer possesses these requirements to a hitherto unequalled extent, and for stationary employment far exceeds all others in utility.

Potential.

2°. Difference of electric potential, or "electromotive force," is thus defined by Sir William Thomson:—"The amount of work required to move a unit of electricity against electric repulsion from any one position to any other position is equal to the excess of the electric potential of the second position above the electric potential of the first." Thus a difference of electric potential takes the form of mechanical work; and it follows also that a positively or negatively electrified body will experience a force tending to move it from a place of greater positive or negative potential to a place of less positive or negative potential. This is the principle of all electrometers. A light, easily-moveable conducting body charged with electricity is placed between two conducting systems, and the difference of electric potentials established between these latter is indicated by the motion of the former. The direction of motion denotes the *kind* of the difference, and the amount of the motion, determined by scale, measures the *amount* of the difference.

Early Forms.

3°. The Quadrant Electrometer is, like all other highly-perfected contrivances, an instance of that process of evolution which goes on among mechanical as well as natural creations. This Prince of Electrometers, so beautiful and complex in its present complete form, had for its progenitor a large iron tank and a three-legged stool. But since that early day (when there were giants) it has gone through several improving modifications. In 1858 it took the form of a divided ring with a light index or rod of aluminium suspended by a fine glass fibre. This was a Torsion electrometer, and used principally for observations on atmospheric electricity. In 1861 the first reflecting electrometer was made and applied to electric testing. Various forms succeeded this till, in 1866, the first Quadrant Electrometer was constructed by Mr. White of Glasgow (who has also brought out all the other forms), with quadrants, Leyden jar, and sulphuric acid—the brain, stomach, and lung analogues of the instrument—all as now.

Electrometers Classified.

4°. Sir William Thomson has classified electrometers into genera and species, according to the shape and kinematic relations of their parts;

but, he says, "as in plants and animals a perfect continuity of intermediate species has been imagined between the rudimentary plant and the most perfect animal, so in electrometers we may actually construct species having intermediate qualities continuous between the most widely different genera."* He has divided them into three classes. (1) Repulsion Electrometers, (2) Symmetrical Electrometers, and (3) Attracted Disc Electrometers. Beccaria's pair of diverging straws, Bennet's pair of diverging gold leaves, belong to the first of these classes; Bohnenberger's gold leaf and dry pile Electrometer and divided ring (including the Quadrant) Electrometers to the second; and Thomson's New Absolute and Portable Electrometers to the third. The same philosopher further says that "there are at present only two known species of the second class, but it (the 2nd class) is intended to include all electrometers in which a symmetrical field of electric force is constituted by two symmetrical fixed conductors at different electric potentials, and in which the indication of the force is produced by means of an electrified body moveable, symmetrically, in either direction from a middle position in this field."

General Principle of Quadrant Electrometer.

This, then, is the general principle of the Quadrant Electrometer, and the practical problem of the instrument has been to carry out this principle by arranging four quadrantal conductors so that they shall contain a symmetrical field of electric force, by maintaining in the middle of this field an electrified needle sensitive to the electric forces of the field, and capable of being deflected by them to an extent proportional to the intensity of these forces, and furnishing the needful qualifications for accurate measurement, viz., a suitable light index and scale by which the deflections of the needle can be indicated by numbers easily read off. The needle must be regarded as the testing body which analyzes the condition of the electric field by its sensibility to the forces of the field; it is the weathercock of the electric atmosphere within the quadrants, endowed with powers of registry, which elevate it to the dignity of an anemometer.

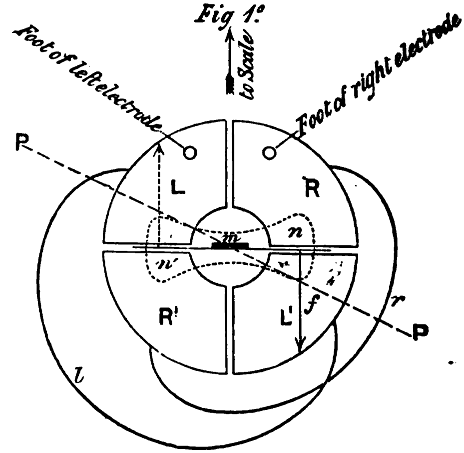
* Papers on Electrostatics and Magnetism, p. 261.

SECTION II.

General Description of Quadrant Electrometer.

1°. As a whole the instrument consists of a cylindrical Leyden jar of white flint glass with rounded bottom, its entire shape being like a conical bullet. This jar is supported upright in a strong brass frame of three legs; the mouth of the jar is closed by a plate of stout sheet brass which is fastened to the brass framing, and can be lifted off at will. This plate of brass is called the "main cover," and all the interior works of the instrument are fixed to and supported by it. Part of the works, as the quadrants and the needle, are below this cover, and part, as the mirror, the point of suspension of the needle and the gauge (of which more hereafter), are contained in a lanthorn-shaped chamber of brass with a glass front, which stands upon, and rises several inches above, the main cover; while the "Electrodes," or metallic rods, by which the quadrants communicate with the outer air, run through the front of the lanthorn and the cover, and rest their feet upon the top surfaces of the two front quadrants. A circular spirit-level is fixed to the main cover; and each corner of a triangular base-plate, with which the three legs of the frame are tied together at the bottom, is fitted with a milled-headed screw as a foot, so that the whole instrument can be truly levelled.

Fig 1°



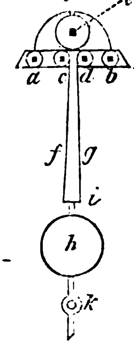
The Quadrants.

2°. The "quadrants," which, together with the "needle," are shown in plan (L L′, R R′, fig. 1°), "consist of four quarters of a flat circular box of brass, with circular apertures in the centre of its top and bottom." Each is supported and insulated on a glass stem, which projects downwards from the cover of the jar, and, as these stems are admitted a certain amount of play in radial slots, the quadrants can, at will, be caused to approach to or recede from each other in a symmetrical manner. Three of them can be directly shifted by hand. The fourth is moved by turning a micrometer screw which gives the manipulator great command over its motion. The quadrants being thus moveable can be brought geometrically symmetrical to each other, and yet be separated from each other, by air spaces, at a common or different distances variable at will within certain limits. The quadrants are *paired* by cross wires (l, r) connecting those which point to each other, so as virtually to form two separate conductors out of the four. Each "electrode" makes good contact with the front quadrant of its corresponding *pair* by a broad silver foot, and, as it is essential that each *pair* of quadrants may be insulated from the rest of the electrometer, the electrodes are encased in vulcanite where they pass through the cover of the jar of the lanthorn.

The Needle, Mirror, and Guard Tube.

3°. The "needle" (n n', fig. 1°) is a thin disc of aluminium, shaped like a double-canoe paddle. It is rigidly fixed to an axis of stiff platinum wire (k, fig. 2°) in a plane perpendicular to the wire. This, together with the needle, is suspended by means of a small cross-piece (i, fig. 2°) at its head from two brass pins (c d, fig. 2°), which project from a brass plate inside the lanthorn, by two single fibres of floss silk (f g, fig. 2°). These two fibres form the "bifilar suspension," which has superseded the single fibre and magnets of the original instruments. The object to be attained is that the needle should of its own accord return to a zero position. In the earlier specimens of this electrometer this was done by a magnetic adjustment similar to that in the mirror galvanometer; but the bifilar suspension secures this in a more advantageous way.

Fig. 2°



4°. Just below the cross-piece the wire carries a light concave "mirror"

of silvered glass (*m*, fig. 1°, and *h*, fig. 2°), firmly cemented to it. The wire and the needle with the mirror rigidly attached form the "moveable conductor" of the electrometer. The fibres *f g* are fastened to the screws *c d*, and can be wound round them at will by turning these screws: thus each fibre can be lengthened or shortened. *a b* and *e* (fig. 2°) are also screws having drilled heads like the screws *c d*, so that the same square-pointed key fits them all. By operating upon *a* and *b* the points of suspension *c* and *d* can be shifted in a plane perpendicular to that of the diagram. The pin or screw *e* is conical; and by turning it the distance between the points *c* and *d* can be regulated.

5°. The lateral deviation of the moveable conducting system is prevented by partially enclosing it in a brass "guard-tube," which freely admits of all necessary movements, but preserves it from being knocked about too much and damaged.

Leyden Jar.

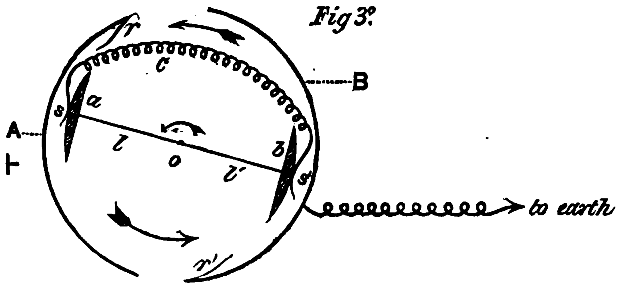
6°. The Leyden jar is coated externally with strips of tinfoil, but for the inner conductor, in addition to tinfoil, a quantity of very strong sulphuric acid is kept in the bottom of the jar. The main use, however, of this acid is to dry the air inside the instrument, and so preserve the insulation of the various internal parts.

7°. The use of the jar itself is to preserve the moveable conductor in an electrified state, and this is done by means of a line and plummet which hang from the end of the moveable conductor, and dip into the sulphuric acid. [This plummet, while it puts the needle at the same potential as the acid, also serves to steady the needle.] And in order that the electrification of the conductor shall be sensibly constant from day to day, it is necessary that the charge of the jar should be maintained the same. To make good, therefore, the small loss of charge, which perpetually takes place and cannot be entirely prevented, the instrument is fitted with a "replenisher."

The Replenisher.

8°. The principle of the replenisher is that of statical induction, the name given to that property by which electricity on a body is enabled to draw forth electricity of an opposite kind to itself upon the nearest parts of any neighbouring conductor, and to drive electricity of the same kind as itself into the parts remote.

Let A (fig. 3°) represent the trace of a curved brass plate positively electrified as denoted by the sign + and connected to the inner coating of the jar; B, another brass plate connected to earth, and so practically



without electrification whatever. Between these brass plates, or "inductors" as they are called, a non-conducting arm $l\,l'$ revolves round an axis O, and fixed to each end of this arm is a small brass plate (a b) or "carrier." In the position shown the "carriers" are in contact with springs s s' which place them by means of a wire c in metallic connection with one another. While so connected A induces negative electricity upon the near carrier a, and drives positive electricity to the remote carrier b. A moment after, as the arm revolves, the carriers breaking contact with the springs are insulated from one another, each, however, retaining its induced charge. Having revolved a little farther, the carrier a comes into contact with a spring r' which leads its negative charge to B and thence to "earth;" while the carrier b delivers up its positive charge through another spring r, to increase that already existing upon A and in the jar. If we look upon the charge induced upon b as being always a fixed per-centage of the charge upon A, it will be seen that the charge upon the latter increases according to the compound interest law, and that a very small quantity of electricity on A, to begin with, very soon developes a large amount. The axis of the replenisher projects above the main cover, and it is turned easily by the finger.

The Gauge.

9°. In order to indicate when the proper constant potential of the needle is reached the instrument is provided with a "gauge." The principle of the gauge is the same as that of the New Absolute Electrometer (par. 4°, Sect. I.), so that the gauge is an electrometer of the third

class. It consists of two metallic discs, having their planes parallel and close to each other. The lowermost is in metallic connection with, and therefore takes the potential of, the jar which is to be gauged. The upper is perforated with a square hole immediately over the centre of the lower disc, and is connected to the outer coating of the jar. A light thin spade-shaped lever of aluminium is connected to and held over the upper disc by a tense and twisted platinum wire, round which it moves as a fulcrum. The blade of this aluminium lever dips into the hole in the disc, and so nearly fills it up that, practically, the upper disc acts as if it were continuous throughout. To carry out the spade analogy in the case of the lever, the rung of the handle is represented by an opaque hair, and inside the handle there rises up a tiny pillar with two black spots enamelled on its face. This hair and pillar form the hand and dial of the gauge indicator; for, as the lower plate acts inductively on the upper, the electromotive force between them will always be an attraction whose intensity will be proportional to the potential of the jar. This attraction pulls the blade of the lever downwards against the torsion of the strained platinum wire which forms its fulcrum, while the hair at the other extremity of the lever moves up across the porcelain face of the dial. The torsion and tension of the fulcrum wire and the distance between the upper and lower discs are so arranged that when the hair is midway between the black spots the proper potential of the jar, and, consequently, of the needle, is attained. This adjustment is of a permanent description, although not necessarily so, and is done by the maker as a rule. The spots and hair are magnified to the observer by a plano-convex lens. In using the lens parallax must be guarded against by keeping the line of sight normal to its centre.

Scale and Index.

10°. The principle of reflection, so successfully applied by Weber to the galvanometer, is also adopted in the electrometer. A beam of light proceeding from a lamp placed behind the scale which stands in front of the electrometer, passes through an aperture placed below the graduated limb of the scale, and falling upon the mirror attached to the needle is reflected back upon the limb. Here it makes a bright spot illuminating the divisions it lights upon. The mirror moves through the same angle as the needle when the latter is deflected, and the beam of light through twice the angle that the mirror moves through. Thus the beam of light

forms the impalpable index of this measuring system. If the aperture in the scale be crossed vertically by a fine wire the spot will be similarly crossed by the wire's shadow, which will thus form the reference line of the moving index. For the steady readings of an electrometer the wire shadow is found superior to the fine line of light so handy in swinging galvanometer readings. The scale is engraved from a copper-plate and is distinctly graduated from 0 to 720 in single divisions, each one millimetre, and groups of five and ten. The scale and lamp are set to suit the focal distance of the mirror, which is usually about one metre (40 inches).

The Induction Plate.

11°. To increase the number of grades of sensibility, the electrometer is provided with an "Induction plate," insulated directly over one of the rear quadrants. It consists of a thin brass plate smaller in area than the top of the quadrant beneath it, and is supported from the main cover by a glass stem. It is provided with an electrode which is guarded by an ebonite jacket. This electrode projects above the main cover alongside of the lanthorn, and is fitted with a binding screw at its head.

Further Details.

12°. Full details as to the mechanism of this electrometer will be found in Sir William Thomson's British Association Report on "Electrometers and Electrostatic Measurements" (1867), now published in "Papers on Electrostatics and Magnetism" (Macmillan and Co).

SECTION III.

Adjustments.

1°. That the electrometer may be in its proper working state the following mechanical conditions require to be made good. The quadrants must be symmetrically placed with respect to each other, that is, their upper surfaces must be in one horizontal plane, and their under surfaces in another plane parallel to it—the perimeter of the whole system being circular—and they should be at equal distances from each other. The needle should be symmetrically placed with

respect to the quadrants, that is, its plane should bisect the depth of the quadrants, and its longitudinal axis, denoted by a dark line upon its upper surface, should bisect the distance between the front and rear quadrants. Withal it is desirable that the two fibres of the needle should be of equal length and stressed equally, although such symmetry of suspension is not absolutely essential to the utility of the instrument.

Adjustment of Quadrants.

2°. The adjustment of the quadrants is done by removing the lanthorn and sliding them together by hand till the eye testifies to their symmetry. The needle is adjusted by operating upon the five screws (*a*, *b*, *c*, *d*, *e*, fig. 2°.) within the lanthorn which regulate its suspension in the manner already described (Sect. II. par. 4°). By combining the action of these screws complete command is obtained over the needle.

Other Precautions.

3°. Besides these mechanical adjustments the glass stems should be clean so as to insulate properly. The works should be free from shreds: and the acid kept powerful, so that the dryness of the air inside the works may be ensured. Also when the instrument is levelled, the suspending wire of the needle should pass down the middle of the guard tube.

Printed Directions for Adjustment.

4°. A pamphlet drawn up by the late Mr. William Leitch, containing clear and minute directions for performing all the necessary adjustments, charging, cleaning the works and preparing sulphuric acid, &c., is sent along with each instrument.

5°. When the electrometer is correctly adjusted and free from all electrification, the "mechanical zero," or division at which the light-index strikes the scale, will be 360. The jar is best charged while the quadrants are all connected. If the mechanical adjustments are very good, on charging the jar the spot will not shift far from the "mechanical zero," because the induction of the needle upon the surrounding quadrants will be nearly uniform. The spot must be brought back to 360 by turning the micrometer screw.

SECTION IV.

Action of Electric Forces on the Needle.

1°. Let us now suppose that the quadrants are at one potential and symmetrically placed with respect to themselves, and that the needle is at rest and symmetrically placed with respect to them; then if we establish a difference of potential between one pair of quadrants and the other pair, the quadrants will enclose a field of electric force, and the needle will be caused to swing round its axis so as to take up a new position of equilibrium. Thus if R R′ (fig. 1°) are electrified to a certain positive potential, and L L′ connected to "earth" (which has practically no potential), then if the charge of the needle is also positive it will take up some new position in the direction of (P P′), for its blade n is acted upon by a force f repelling it from the quadrant (R), and its blade n' is acted on by an equal, parallel, and dissimilar force f', repelling it from the quadrant (R′). These two forces constitute a "couple" which turns the needle about its centre. The mirror turning with the needle reflects the beam of light impinging upon it from the lamp, a certain distance along the scale. This distance, measured in divisions of the scale used, measures the intensity of the electric forces $f f'$, and therefore the difference of potential between the quadrants.

Measurement of Small Difference of Potentials by Quadrant Electrometer.

2°. In using the Quadrant Electrometer to measure a small difference of potentials between two bodies it is only necessary to connect one of the bodies to one electrode and the other body to the other electrode and the case of the instrument conjointly. Thus, to measure the difference of potentials between the poles of a Daniel cell, connect the two separate poles of the cell by a double key to the two separate electrodes, and also connect any one of the electrodes to the cover of the jar by means of one of the binding-screws on the cover. The "induction plate" may also be connected to the cover of the jar. "On putting down the key the image will be deflected over a number of scale divisions proportional to the difference of potentials to be measured, and on reversing the key an equal deflection should take place in the opposite direction."*

* W. Leitch.

Measurement of Large Difference of Potentials by Grades of Diminished Power.

3°. With the instruments as now made one Daniel cell by reversal readings gives about 100 scale divisions; and, in order to measure a much greater difference of potentials, such as that of a telegraph battery, it becomes necessary to use the grades of diminished power. For all the grades "the different methods of forming the connections, with or without an inductor, are indicated in the following table, where R means the electrode of the pair of quadrants marked R R′ in fig. 1° (Sect. II.), L that of the pair L L′, and I that of the induction-plate; C is the conductor led from one of the bodies experimented upon; O the conductor led from the other and connected to the outer metallic case of the instrument, which may be insulated from the table if necessary by placing a small block or cake of clean paraffin under each of the three feet on which the instrument stands; (R) or (L) means that the electrode of R R′ or L L′ is to be raised so as to be disconnected from its pair of quadrants. Thus, in the grade of diminished power or sensibility

Without Inductor
Full power

$$\begin{bmatrix} L & C \\ R & O \end{bmatrix} \ \textit{or} \ \begin{bmatrix} R & C \\ L & O \end{bmatrix}$$

With Inductor

$$\begin{bmatrix} L & C \\ R & O \end{bmatrix} \ \textit{or} \ \begin{bmatrix} R & C \\ L & O \end{bmatrix}$$

Diminished power

$$(L)\begin{bmatrix} R & C \\ & O \end{bmatrix} \ \textit{or} \ (R)\begin{bmatrix} L & C \\ & O \end{bmatrix}$$

Grades of diminished power

$$(L)\left\{\begin{matrix} \begin{bmatrix} R & C \\ I & O \end{bmatrix} \\ \begin{bmatrix} R\,I & C \\ & O \end{bmatrix} \\ \begin{bmatrix} I & C \\ R & O \end{bmatrix} \end{matrix}\right. \ \textit{or} \ (R)\left\{\begin{matrix} \begin{bmatrix} L & C \\ I & O \end{bmatrix} \\ \begin{bmatrix} L\,I & C \\ & O \end{bmatrix} \\ \begin{bmatrix} I & C \\ L & O \end{bmatrix} \end{matrix}\right.$$

$$(RL)\begin{bmatrix} I & C \\ & O \end{bmatrix}$$

standing first in the table on the right, the electrode L is raised, one conductor is connected with R, I and the other with the case of the instrument. The grade standing last in the table in which L and R are both raised is the least sensitive of all."

Induced Charge on Raising Electrode.

4°. If upon raising any one of the electrodes from the pair of quadrants beneath it the light-spot on the scale should be deflected, it means that in the act a charge has been induced in that pair of quadrants. This charge must be led away to earth. For this purpose the quadrants L L′ are provided with a disinsulator, having a milled vulcanite head projecting above the "main cover." When this head is turned (by hand) till a small projecting pin in its side points to the word "contact" (C) engraved on the cover, the image will return to its proper place, and the milled head must then be turned to "disconnect" (D), so as to insulate the quadrants again. It will be seen that the electrode in contact with this pair of quadrants (L L′) should, if possible, always be raised when it is necessary to use the less sensitive states of the instrument.

SECTION V. DIVISION 1°.

USES IN TELEGRAPHY.

Testing Cables for Insulation Resistance.

1°. Perhaps the best method hitherto tried of testing submarine cables by the Electrometer has been successfully employed by Sir W. Thomson and Professor Fleeming Jenkin in the tests of the Great Western Telegraph Company's cables and the Platino-Braziliera Company's cable. It was fully described by Professor Jenkin in a paper to the Society of Telegraph Engineers, read on March 26th, and the following is a brief explanation of the process :—

2°. The use of any electrometer in cable testing is to measure the potential of the cable at stated intervals after its preliminary electrification. From the scale numbers representing these potentials the insulation resistance in megohms is calculated by formula.

3°. Let us now suppose that with a quadrant electrometer at its highest grade of sensibility, and a battery of 100 Daniel cells, we wish to find the Dielectric Resistance, in megohms, of a cable by this mode, at the end of the second and fifth minutes after first electrifying it.

It is necessary that the electrometer should be carefully insulated, as

the case will have to be electrified. The scale of the electrometer, as we know, reads from 0 to 720, and for battery testing and experimental purposes the reading 360 at the centre of the scale, is conveniently made zero, as admitting the most symmetrical disposition of the mirror with respect to the rest of the instrument; but, as we have now to deal with large deflections, it is better to make the mechanical zero 0 instead of 360. This, of course, is done by operating on the screws (*a b*, fig. 2). When the jar is charged and the quadrants are all at one potential the index will, therefore, stand at zero. One pole of the battery is put to earth, and the ends of the cable are insulated. If now the scale was large enough, all that would be required to be done would be to electrify the cable to the potential of the 100 cells, and connect it to one pair of quadrants, keeping the other pair and the case at no potential. Then a reading taken at any time would measure the potential due to the cable at that time. But it is easy to employ the handy scale in use by means of the following device. The free pole of the battery is put to earth through a series of coils of resistance, so great as not to weaken the battery sensibly. These coils subdivide its potential, and if the pole be also led to one pair of quadrants (R R′), so that these quadrants have the potential of 100 cells, while the other pair (L L′) are led to that part of the subdivider which has a potential of 90 cells, it follows that the difference of potentials between the two pairs of quadrants (R R′) and (L L′) will be that due to 10 cells. The deflection so obtained multiplied by 10 will give the reading we should get for 100 cells, provided the scale were long enough to embrace it—which it will not be. The imaginary reading so obtained is called the "inferred zero." It is the reading which would be got if 100 cells were kept upon the first pair of quadrants (R R′), while the other pair (L L′) were kept at no potential. Next charge the cable with the same 100 cells for a time sufficient to allow it to acquire throughout its length the potential of the battery (say 15 seconds), and join it up to the pair of quadrants (L L′) previously electrified with 90 cells. We will now have the first pair of quadrants (R R′) still electrified by the battery to the potential of 100 cells. The spot will, therefore, stand at the mechanical zero. By leakage through the dielectric, however, the potential of the cable is always decreasing, and the light-index will move up the scale nearer to the "inferred zero;" till, when the cable has entirely lost its potential, the light spot will remain stationary at the "inferred zero." The rapidity with which the spot moves up the scale at any time

can, of course, be made a measure of the loss of the potential of the cable at that time, and this loss is a function of the insulation resistance. To apply Siemens's formula, given in Clark and Sabine's Electrical Tables, in order to find the mean insulation resistance of a cable during an interval of t minutes, or, in other words, the insulation resistance at the middle of that interval, it is necessary to take scale readings at the beginning and end of t. Let r_1 and r_2 be these readings; Z the inferred zero. Then $(Z - r_1)$ and $(Z - r_2)$ are at once the deflections of the light spot from Z and numbers representing the relative potentials of the cable at the beginning and end of t; and, if F be the electrostatic capacity of the cable in microfarads, we have the insulation resistance in megohms

$$= 26{\cdot}06 \frac{t}{F\left\{\log.\left(Z-r_1\right)-\log.\left(Z-r_2\right)\right\}} \quad . \quad . \quad 1^{\circ}.$$

To get the insulation resistance in megohms, therefore, of our cable, at the ends of the second and fifth minutes after charging, the following readings on the scale must be taken (if we choose t, as is found very convenient in practice, equal to ½ a minute), viz., for the second minute result r_1 at 1 min. 45 secs., and r_2 at 2 min. 15 secs., and for the fifth minute result r_1 at 4 min. 45 secs., and r_2 at 5 min. 15 secs. The longer the interval t the higher the potential of the battery, and the more sensitive the electrometer the greater the delicacy of this test. "When compared with any galvanometric method it has other advantages. The absence of any magnet in the electrometer allows the test to be made while machinery is in motion. Currents which from various causes are induced in the cable, causing irregular deflection on the galvanometer, are unperceived on the electrometer. Short coils of high insulation resistance, which would show no sensible leakage on the galvanometer, are easily tested by the new method; and last, not least, several coils or cables can be tested at one time with a saving of labour."* This last advantage is owing to the facility with which one cable can be joined to the second pair of quadrants in place of another, the first pair of quadrants being kept constantly at the potential of the battery.

Continuous Cable Test.

4°. As a continuous test, during manufacture and laying, the electrometer may be kept joined to the cable whose ends are insulated. The

* Journal of the Society of Telegraph Engineers, vol. ii. No. 5, page 174.

cable is charged at intervals by a few cells, and the electrometer shows the rate of loss of potential. If this is normal all is well. The credit of this application is due to Mr. Warren.

Internal Resistance of a Battery.

5°. In measuring the internal resistance of a single cell, or, by the use of the grades of diminished sensibility, of large batteries, this electrometer is extremely useful. Thus to find the resistance of a single cell:—

1° step—Connect one pole to one pair of quadrants, and also to the case of the instrument and induction plate electrode on the "main cover": then connect the other pole to the other quadrants, which must be kept insulated. Let d_1 = deflection so obtained.

2° step—Without altering these connections (1°) insert a suitable shunt of S resistance between the poles of the cell. Let d_2 = deflection now obtained.

Then $\left\{\frac{d_1 - d_2}{d_2}\right\} \times S = B$, the resistance of the cell. For since the difference of potential between any two points of an electric current is directly proportional to the resistance between these points, we have the proportion

$$d_1 : d_2 :: B : \frac{S \times B}{S + B},$$

where $\frac{S \times B}{S + B}$ = the joint resistance of s and B between the battery poles. Also from a consideration of the method of finding the resistance of a cell by a convenient galvanometer, such as the tangent galvanometer, it is easy to see how the formula comes. Thus let

d_1 = tangent of angle of deflection produced by the cell without the shunt,

d_2 = tangent of angle of deflection produced by the cell with the shunt,

s = resistance of shunt,

g = resistance of galvanometer,

we have $d_1 = \frac{C \times E}{B + g}$ . . . . . eq. $\propto$.

Where E = Electromotive Force of the cell and

C = constant depending on the instrument,

$$\text{and } d_2 = \left[\frac{C \times E}{B + \left\{\frac{s \times g}{s + g}\right\}}\right] \times \left(\frac{S}{S + g}\right) \text{ . eq. } \beta.$$

and solving out these two equations (L and β) we get

$$B = \left\{\frac{\frac{d_1 - d_2}{d_2}}{1 - \left(\frac{d_1 - d_2}{d_2}\right) \times \frac{s}{g}}\right\} \times s \text{ . . eq. } \gamma.$$

Now the quadrants of the electrometer are disconnected, and its internal resistance is infinite; accordingly, when an electrometer is employed instead of a galvanometer, the quantity g in the formula (γ) becomes infinite, and this equation reduces itself to

$$B = \frac{d_1 - d_2}{d_2} \times s,$$

which is also the value that the proportion above gives for B.

Resistance of Conductor.

6°. In order to measure any resistance R_1 in a circuit by the electrometer, the process, in principle, is as follows: Connect the electrometer (as in par. 2, Section IV., and par. 5, Section V.) to the two ends of R_1, so as to measure the difference of potentials between these ends. Let d_1 = deflection so obtained. Then for R_1 substitute a known resistance R_2, and let d_2 = deflection now obtained. By par. 5, Section V. we have the proportion

$$d_1 : d_2 :: R_1 : R_2$$
$$\therefore R_1 = \frac{d_1 R_2}{d_2}.$$

Measurement of Capacities.

7°. The electric capacity of a condenser of whatever form is measured by the quantity of electricity necessary to charge the condenser so that there is unit difference of potential, or unit electromotive force, between its insulated and uninsulated conducting surfaces. Expressed in formula this becomes

$$C = \frac{Q}{E} \text{ . . . . } 1°$$

where C = electric capacity.
Q = " quantity.
E = electromotive force.

From this it is clear that, if we have two condensers holding each the

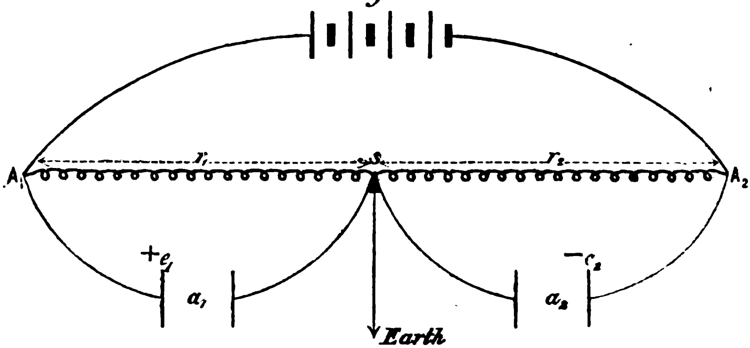
same quantity of electricity, their capacities will be inversely as the electromotive forces between their conducting surfaces.

Thus if C_1 = Capacity of one condenser,
E_1 = Electromotive force between its conducting surfaces,
C_2 = Capacity of the other condenser,
E_2 = Electromotive force between its conducting surfaces,
And Q = Quantity of electricity in each, we have from

equation 1°, $\frac{C_1}{C_2} = \frac{E_2}{E_1}$ . . . . 2°.

With a quadrant electrometer and a resistance slide the capacities of two condensers can be conveniently compared in the following way :—

Fig 4°



In figure 4° let $\overline{A_1 A_2}$ be the series of equal coils in the resistance slide, of which there are usually a hundred. To the extremities A_1 A_2 of this series let a battery be joined up, and let the moveable contact stud of the slide s be connected to earth. The electromotive force or difference of potentials of the battery is divided at the stud s in the ratio of the total resistances between it and the extremities A_1 A_2. Thus, if E_1 be the difference of potentials between the points A_1 and s, r_1 the total resistance between A and s, and similarly r_2 the total resistance between A_2 and s, while E_2 is the difference of potentials between these points, we have—

$$\frac{E_1}{E_2} = \frac{r_1}{r_2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 3°.$$

If now we take two condensers, a_1 and a_2, and connect one plate of each to s, and apply the other plates to A and A_2 respectively, as shown, we shall charge a_1 to a difference of potentials E_1, and A_2 to a difference of

potentials E_2; and, if the quantity of electricity in each condenser be equal, then—

$$\frac{C_1}{C_2} = \frac{r_2}{r_1} \quad . \; . \; . \; . \; . \; . \; . \quad 4^\circ.$$

Where C_1 = capacity of a_1, and

C_2 = " a_2. If the quantities be unequal, the sliding stud must be shifted so as to give such a division of the electro-motive force of the battery, as will charge the condensers with the same amount of electricity.

The use of the quadrant electrometer is to determine when the quantities are equal. It is evident, since the condensers are applied to the two different poles of the battery, that they will be charged with opposite kinds of electricity. If then, immediately after being charged at the points A_1 A_2, the condensers are discharged into one another, the two electricities will wholly neutralize each other when the charges are equal, and, upon being both applied to the electrometer, there will be no signs of electrification detected about them. If a deflection of the light-spot should show a residual electrification in the condensers, conjointly tested in this way, the stud must be shifted and the process repeated till none is shown, then equation 4° gives the ratio of the capacities. The capacities of short lengths of submarine cable core may be accurately compared by this method. The cables must be well insulated, and as little time lost in the operations as possible.

DIVISION II.—USES IN RESEARCH.

Results in absolute Measure.

8°. "The first step," says Sir William Thomson, "towards accurate electrometry in every case is to deduce from the scale readings numbers which shall be in simple proportion to the difference of potentials to be determined. The next and last step is to assign the corresponding values in absolute electrostatic measure. Thus, when for any electrometer the first step has been taken, it remains only to determine the single, constant co-efficient, by which the numbers, deduced from its indications as simply proportional to differences of potential, must be multiplied to give differences of potential in absolute electrostatic measure."* To

* Papers on Electrostatics and Magnetism.

2 B 2

determine this co-efficient for the quadrant electrometer, it is necessary to compare its indications for a suitably constant difference of potential, such as that between the poles of a good battery of Daniel's elements, with the indications of an electrometer or other instrument from which results can be obtained in absolute measure. In other words, measure the battery upon both instruments and deduce the co-efficient. Thomson's new absolute electrometer, now stationed in the Physical Laboratory of Glasgow University, is the best instrument of its kind for this purpose. The theory of this instrument is a particular case of Green's theory of the Leyden phial, in which the plates are plane, parallel, and the distance between them small in comparison with their diameters. By this theory the force of attraction between the plates per unit of area is $\frac{V^2}{8\pi a^2}$ where V = difference of potential between the plates and a = distance between the plates. In the new absolute electrometer this force is measured in grammes weight, and V calculated from it, a being known. From many experiments made upon the Glasgow instrument the electromotive force of a single Daniel cell has been found to be 0·00374 absolute electrostatic units.

Measurement of Atmospheric Potential.

9°. The quadrant electrometer has long been used at the Kew Observatory, in conjunction with the water-dropping collector, as a self-recording indicator of atmospheric electricity. Its mission is to register perpetually the potential of the atmosphere at a certain place.

Water-dropping Collector.

10°. The collector is an insulated vessel containing water, with a long spout projecting into the air. The spout ends in a delicate nozzle, from which a fine jet of the water continually streams off and breaks in spray. This discharge of spray quickly equalizes the potential of the water and that of the air at the point where the spray forms, so that the water vessel has at any time the potential of the air at that time. The vessel is always connected to the insulated quadrants of the electrometer. The scale is a strip of prepared sensitive paper, which moves vertically at a known uniform rate, and the beam of light reflected from the mirror photographs its track upon it. The datum line of the paper is the line the light-spot would trace if the difference of potential indicated was

zero. By this means is registered for any time the kind and amount of the potential of the atmosphere at the place where the water-jet is disintegrated.

Sphere of action of the Quadrant Electrometer.

11°. Such are a few of the common uses of the quadrant electrometer, but its applications are extensive in the whole range of electric science. In the meteorology of the future it seems destined to play an important part, and, doubtless, its aid will be invoked in the pursuit of other branches of human knowledge. Its importance as an instrument of electric research and an appliance of the telegraph engineer, is rapidly becoming recognised throughout Europe and America.

The Chairman said—I am glad to see present several gentlemen who have been more or less engaged in using this instrument, and although the paper has not gone deeply into its value, and the methods of its use, yet I hope gentlemen present will supply that want. I beg to call upon Mr. W. H. Preece.

Mr. W. H. Preece said,—I have listened with much interest to the paper which has just been read, and I am quite sure when it appears in our Proceedings there will be no paper more useful to electricians in general. It is difficult to follow the details and architecture of an instrument of this kind from an oral description, especially when, unfortunately, we have not before us the instrument itself; not that if it were here we should be much enlightened, because it is an instrument which requires careful examination and an examination of every one of its parts. It is destined, I have no doubt, to become one of the most valuable instruments in the hands of electricians, and I question very much whether an electrician's or a telegraph engineer's apparatus can be considered complete, till he possesses one of these exquisite instruments, and no laboratory can be considered worthy of being called a laboratory, till it is found upon its shelves. The principles on which it is constructed are, like those of all really beautiful instruments, excessively simple, and the indications are given by the mere solution of one of the earliest problems taught, viz., the parallel of forces. The indicator itself, the needle, is simply actuated by the electric forces in two directions, and it takes up a resultant position, and by that means we have upon a graduated

scale a spot of light, which gives an accurate indication of the forces present. The instrument, as has been remarked in the paper, is an example of that principle which is now gaining so much ground in philosophy generally, viz., the principle of evolution. It is an excellent example of the Darwinian theory of natural selection, and of the survival of the fittest. All who have experimented in electricity have made use of electrometers of different kinds. The old gold-leaf electroscope has gradually, by this principle of natural selection, at last been created into this quadrant electrometer. I do not think any museum or school of electricity can be considered complete until it has an historical series of instruments, showing the gradual changes by which one form has passed into another. Speaking from experience, I say young hands are very easily taught the value of the electrometer, and the details of the instrument, by being shown the different stages through which it has passed in its life. Hence this Society, when it possesses a museum of its own, which I hope it will do eventually, could not do better than have a series of electrometers, giving examples of this principle of evolution by showing the different stages through which they have passed. There is one part of this instrument which has been rather cursorily passed over in the description, and which really is one of its most beautiful parts, that is, the replenisher, the simple means by which at every movement we are able to keep this instrument up to its proper standard, and by which we are able at all times to obtain comparable and actual results. This replenisher is one of numerous inventions of Mr. Cromwell Varley, and, being here, I have no doubt he will be good enough to give you, in his usual clear way, a description of that instrument. The principle of accumulation on which it was based was explained, I think, by him in the year 1860, and he will show you the experiment on which it was based. Now, the uses to which this instrument has been put are mentioned in the paper. It has to my knowledge been for some time in use in Hooper's Telegraph Works at Millwall. It has this great merit—that testing by this machine you are able to watch with your eyes what is going on. It is not as though you used a galvanometer and were obliged to dodge the current to get the proper indication, but with this instrument you see before you the whole operation. It is as though you filled a can with hot water, and, wishing to know how long it takes to cool, you insert a thermometer into it, which, by the gradual falling of the mercury in the thermometer, gives you the information you desire. So, applying the electrometer to a

cable you can gradually see its potential fall, and you are able to trace with the greatest exactitude the rate at which it falls. There are many practical purposes in telegraphy to which this instrument can be applied, and the members of this Institution could not possibly render to science a greater service than to take indications, at the different stations at which they are placed, of the variation of the potential of the earth, for instance. We are all troubled, more or less, with earth currents, which formed the subject of a paper at the commencement of the present year, and the benefit we derive from the reading of these papers is this, to show how ignorant we really are with regard to earth currents: they are still a mystery to unravel, and a problem to solve; and I am sure if throughout the world telegraphists supplied themselves with an electrometer of this kind, and would take well-considered observations, we should by that means be gradually accumulating those data which form the basis of every science; for it is only by comparing well-observed facts that science is arrived at. So I am sure all who can afford to supply themselves with this instrument, and will take observations of the potential of the earth, will be doing a real service to science in general, and to our own science in particular. Again, there is another vexed subject, that is, atmospheric electricity. We are continually being told something new about lightning, and thunder, and atmospheric storms; but it is all conjecture. In that field we want to accumulate the same facts as to earth currents, to enable us to place atmospheric electricity on the true basis of a science. The instrument itself has two or three practical uses to the student. One is that it gives him a clearer conception of that much-abused term "potential," than any other machine that is placed in his hands. We have heard this evening Sir Wm. Thomson's definition of potential. To many the term is simply incomprehensible, but by comparing it with pressure, as applied to the movement of liquids, and temperature, as applied to heat, we gradually acquire some knowledge of what potential is, as a function of electricity. It is a good thing, sometimes, to play with novel kinds of instruments, and for my own part I can say I never clearly felt what the meaning of potential was, till I was able to play with an electrometer. Another purpose to which this instrument can be used is this. As we all know, we are living in a state of transition with regard to electrical theory. No two men are agreed as to what electricity is. Some say it is a fluid; some say it is a property of the ether; some say it is force; some say this, and some say that; and this of itself is a proof that we

have not yet arrived at the true basis of electricity. In discussing such a simple phenomenon as that observed in a galvanic battery, you find telegraphists and electricians are still divided into two great schools, one attributing the birth of voltaic electricity to chemical action, and others, who ascribe it to contact of dissimilar metals. We know that for the last half-century science has been divided on this subject. Continental philosophers generally say that the origin of electricity is to be found in contact. We English electricians have followed the doctrine of our own Faraday, and ascribe it to chemical action; but the tendency of the philosophy of the day is to make a compromise between the contact theory and the chemical theory. Helmholtz in Germany, and Thomson in England, have started the theory that the first kick given to the ball is contact, and that the rolling is really kept up by chemical action; in other words, that the first action of the battery is not due to chemical affinity between the liquid in the battery and the plates, but to the contact of dissimilar bodies. Now, the electrometer enables us to perform, with the greatest ease, experiments which enable one to see there is such a thing as contact electricity. De La Rive has devoted a long chapter in his work to show that there is no such thing as contact electricity. He ascribes it to the moisture of the fingers or the atmosphere, or some such cause, which might produce chemical action. With this electrometer, by placing two dried plates together, one of copper and one of zinc, you see at once, on disconnection between the two, that there is strong evidence of electricity. Several other experiments with this instrument might be mentioned, which will prove that there is contact electricity, and that it is the true basis of voltaic electricity; so that in all future treatises on electricity we shall probably find that this theory, which is gaining ground, will be adopted as the basis of the action in the battery. I cannot do better than conclude by expressing a hope that members will supply themselves with this electrometer, and will make observations to enable us to place on a true basis of science facts connected with atmospheric electricity and earth currents.

Mr. Andrew Jamieson said—The plan which has been adopted by Sir W. Thomson and Professor Fleeming Jenkin in testing the insulation resistance of the Platino-Braziliera Submarine Cable by the quadrant electrometer is as follows: A battery of 100 Daniell's elements is used, one pole of which is connected to earth, while the other is connected with a set of 100 resistance coils (each coil having a resistance of 100 ohms), and, the last coil of the set being to earth, the battery-

current makes a complete circuit through the coils. These coils are encased in a box, with terminals from each coil projecting in a straight line along its whole length; underneath and parallel to these terminals is a brass rod upon which slides a contact-maker, so as to connect the brass rod, and thereby the quadrants of the electrometer, with any one of the terminals by which the battery-current in passing through the resistance coils may be run off at any potential between that of 100 cells and zero, either to charge the cable with full power, or on the other hand to lower the potential of one pair of quadrants to suit the limited range of the scale for the falling potential of the core or cable under test. Immediately in front of the box containing the resistance coils is placed the electrometer, and from each of the electrodes, connected respectively with the pairs of quadrants marked L L′ and R R′ in Mr. Munro's figure, is a wire to the opposite sides of a commutator with one plug, one side of this commutator being connected to the brass rod before mentioned and therefore to the battery, while the other side is led to the cable or core to be tested. When about to take the insulation resistance test by the electrometer, the first thing, after seeing that the connections are right and everything in good order, is to take what is called the "inferred zero," or, in other words, the deflection which the full battery power would give upon the scale; this scale, which is placed in front of the electrometer at the proper distance, is about 20 inches long, and divided into 1,000 equal parts. This deflection cannot be ascertained directly owing to the limited length of the scale, so the contact-maker is slid along the brass rod to the terminal of the 20th resistance coil from the end at zero potential, and the deflection noted when the plug of the commutator has been taken out and one side put to earth, while the other is kept in connection with the battery through the contact-maker; this deflection has to be multiplied by 5, to give the true inferred zero, or the deflection which the full battery would have effected. Two cables or lengths of core are generally tested at the same time, but if we follow out the plan of taking one it will be more clearly seen. Have the light on the scale at zero, the contact-maker at the end next the battery, so as to charge with full power the plug in the commutator; now charge the cable and quadrants for fifteen seconds, take out the plug of the commutator, thereby disconnecting the battery from the cable; the charge of electricity which the cable received during the fifteen seconds now leaks through the dielectric, the needle-mirror is deflected, and consequently the spot of light deviates from

the zero of the scale. Ninety seconds after disconnecting the cable from battery, take the first reading, and thirty seconds after this take the second reading, both of which are placed in column 3 of the test form. We now subtract the readings from the inferred zero, placing the results in column 4, and by these two differences from the inferred zero is calculated the insulation resistance per knot by the formula mentioned in Mr. Munro's paper, viz.,

$$R = \cdot 4343 \frac{t}{F \log. (c_1 - c_2)},$$

where (R) is the insulation resistance in megohms, (t) the time between the two readings in seconds, (F) the electrostatic capacity of the cable, (c_1) the first difference from the inferred zero, while (c_2) is the second.

Date, November 12th, 1873. Hour, 9 p.m. $\frac{1}{5}$th Battery=600.·. Inferred zero=3000.

Example:—

Cable or Coil.	Minutes and Seconds of Test.	Readings.	Deflection from Zero.	Logs.	Difference of Logs.	Resistance Megohms.	Resistance Megohms at 75° F.	Per cent. Charge.
1	2	3	4	5	6	7	8	9
—		190	2810	3·44871				
	15·30	242	2758	3·44059	0·00812	4560	855	91·9

In the example we have chosen we suppose 600 to be the deflection when the contact-maker was at the 20th terminal from zero end of the resistance coils, and therefore the needle-mirror deflected with a power $=\frac{1}{5}$ of the whole battery, and thus the inferred zero is 3000. The first reading is 190, the second 242, their differences from the inferred zero 2810 and 2758; their logarithms are found in column 5, while column 6 is log. $(c_1 - c_2)$, or the difference of the logarithms in column 5; and, supposing F the electrostatic capacity to be 0·352 microfarads, we have 4560 megohms as the insulation resistance. At observed temperature, which if multiplied by the co-efficient for difference of temperature from 75° F. would give 855 megohms as the insulation resistance reduced to 75° F., the °/₀ charge, as seen in column 9, is found from the proportion—as the

deflection from 100 cells : deflection from charge remaining after 2 minutes' insulation : : 100 to the °/$_0$ charge, or

$$\frac{2758 \times 100}{3000} = 91{\cdot}9 = {}^\circ\!/_0 \text{ charge.}$$

In the case of testing core at 75° F., or a cable of comparatively small insulation resistance, the readings (column 3) are not subtracted from the true inferred zero, but from the deflection which would take place if one pair of quadrants were connected to earth, and the other pair to the contact-maker at the position where the readings were taken; which is found thus. Suppose, in order to obtain a reading on the scale with a fast leaking core, we had to slide the contact-maker down the 60th terminal from the zero end of the resistance coils, we should subtract the readings found, from $\frac{1}{10}$ the true inferred zero multiplied by 6, and taking the true inferred zero of our former example the readings would have to be subtracted from $\frac{3000 \times 6}{10} = 1800.$

It will be thus seen that the electrometer enables us to test cables of whatever insulation and at any time after being charged. Careful tests taken of the same cable with the electrometer and galvanometer coincide wonderfully, while the former plan has many advantages to recommend it.

The CHAIRMAN—This paper is one of those which deserve from this Society great consideration, and the author of the paper deserves our warmest thanks for giving us a written description of an instrument which is coming more and more into use every day. When the testing of a cable by the diminution of the charge left in it was first proposed, it was almost universally ridiculed, like all good things are. Now, that dates back to about the year 1848, when it was first proposed, and, like the electrometer, it has gone through a series of developments, until it has been worked into such a state that it is now incomparably the best method of getting at the electrical condition of a conductor. It is important to all electricians that an accurate and intelligible description of such apparatus, and the mode of using it, should be stated in such a manner as to be easily intelligible to those who wish to make use of it.

The great merit, or one of the great merits, of testing the insulation of cables by the electrometer is that you do not interfere at all with the charge in the cable. Having charged the cable, the electrician simply looks at it, just as with a telescope a person reading from a distance the

time upon a clock-tower does not interfere in any way with the working of the clock, but simply regards what is going on, and, as by the end of the telescope you are able to see what the time is, so the electrometer does not really touch the cable; consequently your cable is entirely free from all disturbances, such as those due to battery potential, or battery resistance, or resistance of the galvanometer, or imperfection of the galvanometer; therefore, the electrometer method of testing, being so much freer from these disturbances, is incomparably better wherever it can be used. I do not know yet whether the electrometer has been so far perfected as to be available on board ship in a storm, but the day is not far distant when that difficulty, if it has not been already overcome, will be so. The method which Mr. Jamieson described as being adopted in testing the Brazilian cable was brought before this Society last year by Professor Jenkin, and I take a little pride to myself in having been the first to propose that method of testing. You will find it published in a patent of mine in 1861.

Referring to the reflecting electrometer, when you deal with such minute variations of potential as a fraction of one cell, the surface of the metal itself will be found at times to give some inconvenience from producing charges on its own account. For instance, if a piece of metal is oxidized its potential immediately changes, and therefore I have found it convenient, and it costs very little more money, to have all the parts of the electrometer electro-gilt. By doing that the instrument is greatly improved, and I recommend those who have reflecting electrometers to go to the small additional expense of having all the parts electro-gilt. There is only one more point to which I will refer, although it is somewhat wide of the paper, but Mr. Preece has alluded to it. That is as to the source of electricity—is it contact, or is it chemical action? The position I take upon that question is, that contact alone will not do it, and that chemical action alone will not give you electricity. You must have both.

In conclusion, I wish to remind members how advisable and necessary it is that they should contribute papers—no matter how long or how short—to be read before the meetings of this Society. What we require especially is, to receive papers from gentlemen who are practically working with cables, because every day new things are arising—new difficulties are being met with, and new methods of overcoming them are being devised, and it is most important that these should be put on record for the benefit of those who come after us. I would state to gentlemen

who may be diffident of reading their own papers, that our Secretary will be happy to take that duty off their shoulders.

There is one duty which we must not omit, that is, to pass a vote of thanks to Mr. Munro for the paper which he has given us upon an abstruse and difficult subject, which he has treated in a manner that will read better in the printed volume than is able to be understood from the oral description given to-night. I am sure you will agree that our warmest thanks are due to Mr. Munro for the paper he has contributed to us.

The meeting then adjourned.

The Nineteenth Ordinary General Meeting was held on Wednesday, the 26th November, 1873, Mr. CROMWELL F. VARLEY, F.R.S., Member of Council, in the Chair.

The SECRETARY read the following communication from Mr. C. Neilsen, Director-General of the Norwegian Telegraphs:—

"Christiania, 13th Nov., 1873

"SIR,—I beg to acknowledge the receipt of your letter dated 13th last month.

"I feel much honoured by the offer bestowed on me to be named 'Local Honorary Secretary' to the Society.

"I take this opportunity to send you a specimen of three-twisted No. 11 as we use it in some of our exposed lines in the North. The wire was melted by lightning this autumn, in the same style on different places for a distance of 20 poles—many of the insulators destroyed and poles split. We have not before observed so complete a melting of wire by lightning as in this instance. I have thought it to be of some interest to see this effect of lightning, as I am aware by your Journal that it has been stated No. 8 wire never has been melted when used as earth-wire on the poles. This wire was the real line-wire.

"I remain, &c.,

"(Signed) C. NEILSEN."

The CHAIRMAN: Has any gentleman any remarks to make upon this short paper? The pieces of wire referred to are being passed round the room. You will observe that the points where the three wires are struck are fused into one mass for the distance of about $\frac{3}{8}$ths of an inch.

Mr. W. H. PREECE: I may just mention that the statement referred to by Mr. Neilsen was made by myself. What I said was, that No. 8 wire, when used as earth-wire, never fused; that is quite correct. The instance now before you is that of line-wires, and I have myself known instances of No. 8 wire, when used as line-wire, being fused.

The CHAIRMAN: I remember one instance being reported to me by one of my superintendents on the Chester and Holyhead Railway of a piece of No. 8 wire being fused near Conway, and he sent up the two ends to

Telegraph Street. My reason for speaking of that is this:—If a spark of lightning enters a wire, the action at the point of contact, if I may use the term, between the spark and the wire, is very much greater than the heat-producing power throughout the rest of the circuit; and you find that a very small wire will carry a charge through it, which, striking from that wire into another wire, will fuse it. It is very well known to all who have used voltaic batteries, that, if you take two wires in your hand and make the arc between them of thick pieces of wire, the two ends will immediately get red-hot, while a very small wire is sufficient to carry the charge through, provided there is no break. And the fact that a wire has been fused does not show that it would not be strong enough to carry off the charge when struck on the end; and Mr. Preece is quite right in stating that no earth-wire has been known in his experience, or in my own, being fused by a flash of lightning, or could be fused at the point where the spark entered in.

The following paper was read—

ON UNDERGROUND TELEGRAPHS.

By Mr. G. E. PREECE (Secretary).

It would be almost hopeless to exhaust the subject of Underground Telegraphs in one evening, and, although the title of this paper is a general one, it is intended to consider only one especial underground work, carried out under different circumstances to any previous one; and it is to be hoped that, in the discussion which may follow the paper, some interesting details may be brought out of other works of which we have to the present time no record.

The special work alluded to was the laying down of a system of underground wires along the main road between Manchester and Liverpool, consisting of 14 wires, for a length of 36 miles. Before entering into a description of this work it may not be uninteresting to call attention, in some slight degree, to one or two previous underground extensions.

Of the various underground extensions in England (it is needless here to refer to the large system established and abandoned on the Continent), they all start from about the year 1853, and consisted generally

of the same form, the majority being laid by Mr. Henley on his own plan. The Manchester and London wires for the Magnetic Company may be taken as a specimen of the whole. A second form was that carried out by the Electric Telegraph Company, in 1853, in laying down their express wires from London to Manchester and Liverpool. A third form may be exampled by the underground wires laid by Mr. West in the Isle of Wight, 1856–7.

The system adopted in the express wires was on the pattern of all our street work, and consisted of placing pipes (iron and earthenware) underground on the railway, and subsequently drawing loose cables in. The cables were of eight No. 3 gutta-percha wire, taped, tarred, and sanded. The cables were in quarter-mile lengths, and test-boxes placed at every mile. The failure of the work came about very soon, and briefly may be said to be due to—want of any system of testing; work hurriedly done; economy and speed studied at the expense of care and safety; wires exposed in the open air and in the sun for days, and even weeks—the last being the most prolific source of faults.

The Magnetic Company's London and Manchester Underground Wire.

This work was commenced and completed in 1853, and consisted of a cable of 10 wires, laid in grooved boarding. It started from London, and proceeded by the high road through the principal towns to Manchester—a length of a little under 200 miles.

The wires were of No. 16 copper, insulated with gutta-percha to gauge No. 3. The ten wires were placed parallel to each other, in two rows, and served over with two servings of jute soaked in Stockholm tar.

The boarding used was of Baltic timber, 3½ inches square, creosoted, and grooved by a special machine; a piece sawed off the bottom was used as a cover.

The boarding was placed at the bottom of a trench two feet deep, and the cable was laid in and then covered with the top, which was *nailed* on. The cable, however, was not run in direct; it was coiled in lengths of about a mile and a half on a drum placed on wheels; the end of the cable was made fast to a gate or other convenient place, and the carriage

drawn forward by a horse. When a convenient length, half a mile, had been paid out along the road, it was lifted up and placed in the boarding.

Where iron piping was required, split pipes were used.

At the end of every one and a-half miles, and also where it was required to make a set of joints, test-boxes were inserted, 2 feet long by 3 inches deep, at the same level as the boards. Where boarding was used these boxes were wooden; iron, with iron piping.

The tests of the cable were taken both for continuity and insulation.

Continuity.—Wires were looped at opposite ends and a current sent through.

Insulation.—In the usual way, but with very small power and inferior instruments.

All the wires were numbered and marked at their ends with lead numbers.

Before the work was finished a great number of faults were found out and had to be removed; the generality were due to fixing the top of the boarding with *nails*, these being driven in carelessly frequently penetrated the gutta percha. A long length of line had all its nails removed and the boarding bound with No. 16 wire.

A few years after completion of the work wires were continually failing; when a fault was located in any one cable, the cable was opened and a good wire substituted for the faulty one, the whole cable being restored to the boarding.

Subsequently the line was tested in five-mile sections, any section being condemned was replaced by an overhead line and the underground cable removed. This plan was carried on throughout the whole extent of the line. Finally it was endeavoured to make six wires good throughout; but in doing this a gradual replacement of the line by an overhead one went on, until at length none of the original work was left. In addition to the replacement, the line was actually renewed in many places.

The work itself may be said to have been well carried out; the paying cable out in the road and then placing it in the boarding ought to have been avoided.

The faults which appear to have caused the abandonment of these gutta percha cables were—

1. Cables lying in dry, sandy places.—Gutta percha dried up and contracted.

2. In dirty stagnant water.—Gutta percha became porous and rotten.

3. Fungi, especially near oak trees.—An appendix is attached, giving some interesting reports on this subject.

4. Near gas-pipes.—Gas water, &c., penetrates and attacks the gutta percha, splits the percha, and opens it right to the wire; these splits were longitudinal as well as transverse.

5. Manufacturers' and working faults.—Many of the latter being occasioned by cable being too near the watch-fire.

6. Gutta percha rotting under lead numbers.

7. Pricking wires and omitting to seal places up.

This work has been referred more to, as it may be termed a fair sample of the various lines belonging to the Companies in England differing essentially from the Electric Company's system, the latter drawing wire into pipes, the others placing them in grooved boards and covering them over.

The underground work in the Isle of Wight consisted of bare India rubber covered wire, put down without any protection whatever. The wires were abandoned in a few years afterwards.

The British Company's wires from London to Liverpool were abandoned in a very few years after being laid; one of the six wires never worked at all.

The causes of failure in the Express wires have been eradicated in the present work, and the main cause of failure in the British Company's road wires will, it is hoped, be also avoided; this is a question that time alone can solve; but in the plan adopted in carrying out the work differences will be apparent.

This is the first time that any severe and searching system of testing has been adopted in underground telegraphs, and the result of the care and attention paid to this special part of the work will become manifest.

Route and General Account of Mechanical Work.

The line selected for laying this system of underground wires was from Liverpool to Manchester, and the surveys and selection of the exact route were made by Lieut. Ramsay, R.E., under whose charge also the pipes were laid.

The exact route is about the best that could possibly be selected for an underground line. The new work commences at the "Old Swan,"

where the Liverpool Street work ceases, and proceeds along the main road to Broad Green in iron pipes—(throughout this length the underground work replaced an overhead line). From Broad Green Terminal Pole (where the overhead wires commence again) earthenware pipes were laid as far as Sankey Bridge, near Warrington, the pipes passing through portions of Huyton, Roby, Bold, Tarbock, Cronton, Farnworth, and Penketh, all bye-roads, and coming into the main road near Sankey Bridge. For passing under the canal at Sankey a lead pipe was used, and from there iron pipes were used through Warrington to the outside of the town where the earthenware pipes commenced again. The route from Warrington was along the old main road to Eccles, and from thence to Manchester by the Eccles New Road. The earthenware pipes commence at the Warrington Cemetery, and, passing through the various villages on the road, terminated at a point called Harrison's Bridge, in Barton (six miles from Manchester). From that point iron pipes were used right up to the York Street Office in Manchester.

From Huyton, about seven miles from Liverpool (Canning Place), the ground generally lies very low, and in a part of the country celebrated for its rainfall; even in the middle of summer there was a large amount of rainfall, and, as a rule, there was water in the pipes; this lowness extends right through Warrington; and from that town to Manchester the level is as low as it can well be. The road passes close on the one hand to Chat Moss and the various mosses, parts of the road having evidently a mossy foundation; and on the other hand follows nearly the course of the Rivers Mersey and Irwell, from which it is separated generally by low lying fields and meadows. Throughout this length the pipes had always water in them, and, in such portions of the road as presented a slight incline, a stream of water was constantly running through. Indeed, in one place, a joint box was almost always unapproachable from the quantity of water on the ground. Under ordinary circumstances it was a matter of time to bail out sufficient water to clear the box.

With such an amount of actual water in the pipes, even in summer, a more favourable situation for gutta percha covered wire cannot well be imagined.

The length of the work was:—

Liverpool to Warrington . . .	17·867 miles
Warrington to Manchester . . .	18·257 ,,
	36·124 statute miles.

Of this a length of about 10 miles was in new iron piping and four in old, 22 miles being in stoneware pipes.

A short length of lead piping, under the canal at Sankey, was rendered necessary by the state of the water, containing so much chemical waste, its action on iron being very rapid.

The depth at which the pipes were laid was one foot for the iron and two feet for the stoneware pipes.

The iron pipes previously to being laid were cleaned out with a heavy iron chain, for the removal of any sharp points or excrescences that might damage the cable. The stoneware pipes were also cleaned out for the same object, but with them a rod with a split iron end was used, two pieces of iron like a half pipe kept apart by a spring.

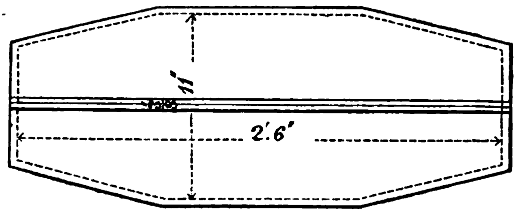
The pipes as laid were carefully adjusted so as to fit close, and the joints were made; as each pipe was laid in its place a No. 16 galvanised iron wire was threaded through; in some places a larger wire was used. The length of wire was in each case that of the pipes to the next box. However, when the work was finished, there were thirty-two miles of iron wire in the pipes, broken only at each box. In the iron pipes the socket was first prepared by ramming in some yarn to prevent the molten lead running into the pipe; a clay mould was then formed round the pipe and the lead run in. The quantity of lead used for this purpose was, according to the contractor, from $\frac{3}{4}$ lb. to 1 lb. per joint.

In the stoneware pipes, which were made in 3 ft. lengths, and 3 in. diameter, the joints were made with Stourbridge clay, which, whilst making a good joint for the prevention of dirt, &c. entering the pipe, was sufficiently porous to allow of water percolating.

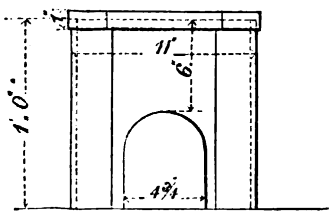
At the distance of every 200 yds., in straight lines, were placed boxes, in the street-work called "flush" boxes—of the old pattern—from being level with the surface, and in the underground work called "coffin" boxes, from their peculiar shape (see diagram); these boxes were of cast iron, with lids fitting over them; they were buried, and had about 1ft. of earth over them.

The cables being divided into lengths of 400 yds., it will be seen that the boxes at that distance would become "joint" boxes where the joints between the two sets of cables would be made. These boxes were invariably placed at the distance of 400 yds., or as near as circumstances would permit, no excess in the length being permitted. The intermediate or drawing-in boxes were placed, when in a straight line, at 200 yds. from a joint box, or halfway between the joint boxes; but where there were

SKETCHES OF JOINT BOX.

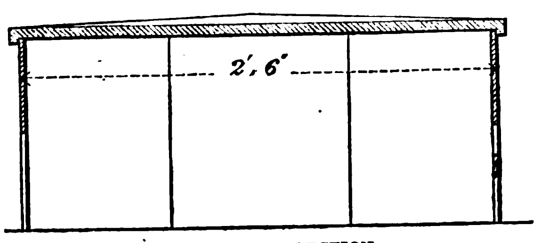


PLAN OF COVER.



SIDE ELEVATION.

SECTION THROUGH COVER.



LONGITUDINAL SECTION.

DIMENSIONS OF CAST-IRON JOINT BOX FOR UNDERGROUND WORK.

Length...	2 feet	6 inches.	
Depth ...	1 „	0	„
Width ...	0 „	11	„

¼ inch thick on all sides. With opening each end to receive 3 inch iron or 3 inch earthenware pipe, and cast-iron cover to fit over top 1 inch deep, ⅜ inch thick, with one rib in centre of cover, the entire length to project ½ inch at centre, and ⅜ inch thick.

curves in the road, obstacles, or it became necessary to make a cross, these intermediate boxes were increased in number as the occasion required.

The pipes were led into the boxes so as just to project within, the spare space around them being protected so as not to allow dirt to fall into the box. The mouth of each pipe was stopped up to prevent the ingress of dirt.

Some difficulty had been experienced in a previous underground work, from it being almost impossible to discover the "joint" places where the line was entirely buried. Every joint box in the open was carefully measured, and the distances taken, but at every *fifth* "joint" box a special mark was made, these were christened "tombstones;" they were of paving stones 30 inches × 9 inches × 3 inches, with the letters G. P. O., and the number cut deeply in. During the work temporary marks were used consisting of coloured wooden pegs.

Form of Cables.

The various forms of cables hitherto used for underground work have differed in the size of the wires, but very little in their form of construction. Of the previous cables there may be said to be two forms: the general form of street wire cables consisted of a number of single wires covered with tarred tape bound together at intervals, forming a cable so called.* The form used by Mr. Henley in the works carried out by him consisted of a number of "plain" gutta percha wires lying parallel to each other, and of the same length, served over with tarred tape or yarn.

The cables used in the present work were manufactured by the Gutta Percha Company, and were entirely different in their construction, and were of the *true* "cable" pattern, and somewhat similar to the wormed and served core of a multiple wire submarine cable.

The core itself consisted of gutta percha wire, No. 7 gauge—

Copper wire . . . No. 18 gauge = 39 lbs. per statute mile;
Gutta Percha to No. 7 „ = 46 lbs. „ „

and was manufactured in the ordinary way, being covered with two coats of Chatterton's compound alternately with two of gutta percha, representing a total weight of 85 lbs. per statute mile.

The core as it was manufactured was cut off specially for the length of cable required, *i.e.* 404 yards. The wires so cut off were wound on to bobbins, which were placed in a machine which held six bobbins only.

* The binding was cut as the cable was drawn into the pipes.

The bobbin containing the centre wire, usually in one long length, as it was unnecessary to cut this wire, was placed behind the machine through the centre of which the wire passed. The machine being started as the ends were passed through the die, cabled up the wire into proper cable form, that is, laid the six outside wires "helically" round the centre, so that each of the outside wires made one turn round the centre wire in 7 inches, and represented a greater length than the centre wire.

The cable so made up was subsequently passed through a bath of cold Stockholm tar, in which was a quantity of fine cork-dust to give the tar more solidity ; after receiving this coat of tar (the superfluous quantity being rubbed off) the cable received two servings of tarred tape laid on in opposite directions. The tape used for these cables was specially manufactured for the purpose, the quality was strong and suitable, and the tape had a double selvage, an important quality.

With the exception of four experimental cables supplied by the Silvertown Company, the cables as described were used throughout the work, the greatest care being taken in their manufacture.

The following shows the strength and elongation of a single wire and of the whole cable :—

1 Piece Single No. 7 Wire.

Weight in lbs.	Elongation per cent.
51	1·4
79	11·2
84	17·5
87	25·2

At which weight it broke.

A similar trial was made with the 7-wire cable :—

Weight in lbs.	Elongation per cent.
308	0·7
364	1·4
420	2·8
476	4·2
532	7·0
560	9·1
588	12·6
616	15·4
644	22·4

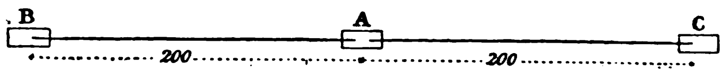
At which weight it broke.

Drawing-in Cables.

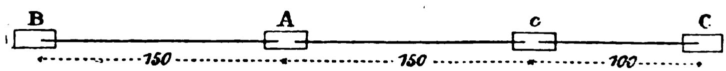
In the first place the holes were opened and the boxes cleaned out; a strong drawing-in wire of No. 11 gauge was attached to the No. 16 wire left in the pipes; the No. 16 wire was then drawn out and the No. 11 drawn in for the entire length. To the end of this wire, in which a loop had been formed, were attached the several cables. The attachment was made by stripping the gutta percha off the copper of each wire for some inches, care being taken to keep all the wires the same length; the copper wires were then passed through the iron loop, bent back, twisted, and secured; the whole of the ends were lapped over with tape and yarn to prevent abrasion.

The cables ready for drawing-in were placed upon "drums" or "swifts" revolving on a stout frame, and at a convenient distance from the mouth of the pipe to avoid friction against the various points. Close to the mouth of the pipe was placed a wooden roller so as to prevent any friction against the edges of the pipe. At the opposite end of the box was placed a mat for the cable to touch instead of the ground, in order that no dirt might be carried in.

Drawing-in cables in a straight length would commence at the centre intermediate box, and the cable would first be drawn through to B then to C;

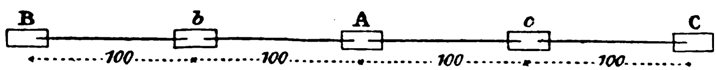


but in a case where there were several boxes intermediate the drawing-in would have to be done once oftener.



The cable would first be drawn in from A to B, then from A to *c*, and then from *c* to C.

In the case of an extra number of boxes the drawing-in has to be done yet oftener.



One-half of the cables would have to be drawn through from A to *b*, and then again from *b* to B; the other half would then be drawn through

from A to C, and again from C to C. When a break occurred the wire was drawn out and laid along the trench to measure the distance at the break, an allowance being made for the spring back; the trench was opened, and, in the case of the pipes being iron, a pipe was broken* (in the earthenware pipes there was no necessity for breaking; by opening the ground so as to expose three joints the pipes could be lifted and any single length drawn out without injury); a wire was then, if the length was short, threaded through to the hauling-in box; if the length was too great, a wire was threaded in from the break and also from the box. By hooking the ends, and, when sufficiently far through, giving the wire a circular motion, the ends were almost sure to catch; the wires were then drawn out and attached to the broken wire of the cable. It was remarkable how very short a distance, even in an empty pipe, it was possible to thread a No. 8 wire.

The drums being placed so as to give a straight lead to the pipe, the roller being fixed and all ready, men being stationed to attend to the drums and another to attend to the lead, the actual hauling-in of the cable would commence; care, however, was taken that where the distance was great, and the boxes not fairly visible from each other, a signalman was placed between the two parties.

In towns or narrow streets it is necessary to "pull and haul" and immediately coil the wire on a drum, but in the open country the work becomes easier and better, for the men shoulder the wire and walk away with it as far as they can, returning again and again until the cable is through the pipe. In such a case the iron wire is run on to its drum afterwards.

Each intermediate box represented the drawing-out of all the cable, minus the length left in, turning the cable over, and again drawing through until the ends came out of the last box. The cable as it came out of the pipes was of course protected by a roller from any friction against the edge of the pipe, and then carefully coiled on to a sheet of canvas; it then, preparatory to being drawn in again, was "turned" over by being coiled down on to the opposite side of the box on to canvas —its lead was then fair for the mouth of the pipe.

In the case of a number of intermediate boxes and a quantity of wires, the drawing-in the wires—drawing-in and out cables—coiling and

* Whenever it was found necessary to break a pipe, a slip-joint was used to protect it; two half-pipes of greater diameter were passed, one under, and the other over, the break; they were screwed up together tightly and the ends packed.

uncoiling, represent a great length of time, and, in consequence of the traffic and inquisitiveness of people in towns, the time occupied is much increased.

After the cables had been drawn in and the numbering wires seen to, the several ends were opened-out, cleaned, and properly prepared for the jointers, who followed on as soon as they had completed the preceding set of joints.

The equipment of each jointer consisted of a properly fitted box, a tent, two stools, and a spade for opening the holes.

Each jointer had an assistant, who facilitated his work very much; the men employed at this were more intelligent than the ordinary labourer, and became very handy. They continued at that especial work until the line was completed.

After the cables had been passed, the van was followed up by two men, whose duty it was to make the cables good. They were provided with a pail containing cold Stockholm tar, and also with tarred tape. The joints and wires were well coated with tar, then laid up and carefully wrapped over with tarred tape, served over twice; the hole was then filled in, the ground being well punned, and all made good. The whole of the cables were served like this, so the jointed portions resembled the main part of the cable.

Electrical Arrangements.

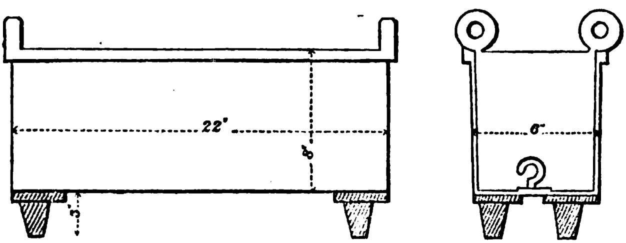
It was necessary that as the works extended over open roads and for some distances a travelling testing-van should be constructed, so as not only to carry all the instruments and batteries, but be able also to travel easily and afford accommodation for more than one person; indeed, to act not only as a testing-room, but also as a travelling carriage, an office, and a day living-room.

The van as depicted in the photographs was designed specially to meet all the various requirements of a lengthened land cruise, and it was found admirably adapted for every purpose. Altogether, it performed a road service of about 1000 miles without a single break-down or mishap.

The electrical apparatus consisted of one hundred No. 3 Leclanché batteries, especially mounted on well-insulated troughs. As the tests of the cable were to be almost identical with the ordinary tests of a submarine core, the insulation of batteries and instruments was carefully

looked to. The instruments consisted of resistance coils, various keys, and very sensitive galvanometers, the principal of which was a small Thomson galvanometer specially constructed for the work, fitted with several improvements to meet the difficulties of great vibration and liability to damage on the road, as well as to be quick and easy of adjustment. This galvanometer answered so perfectly that under all circumstances its use was much preferred to that of the delicate horizontal astatic galvanometer carried in the van, its adjustment being quicker.

It being necessary to test the various joints throughout the work, a special joint-trough was made for the purpose. For placing inside boxes it was provided with ebonite feet to ensure good insulation, and, in cases where it might be necessary to suspend it, eyes at each corner were made; ebonite shackles accompanying it to improve the insulation. Two hooks were attached to the bottom of the trough to keep the joints in their place.



INSULATED JOINT TESTING TROUGH.

For carrying out the tests it was necessary to make "earth" wherever the van stopped; for this purpose a copper plate was carried for such places where water might be convenient, and also a long iron tube (a hollow crowbar), pointed at the bottom end, and with a screw connection for the earth-wire at the top. This rod was driven into the ground in the dampest places, and answered admirably. The end was subsequently perforated, and a cup inserted near the head, so as to allow of water being poured in to increase the dampness of the earth. For testing in the streets an iron clamp was made with a terminal on to it. This clamp was screwed on to one of the lugs of the iron box after it had been well filed, but it was generally found that the best earth was obtained with the "earth-rod."

The testing the several cables for insulation and for copper resistance was a comparatively easy matter, but the proper method of testing the joints was a question of some difficulty. This had to be done soon after the joints were made, and to be done without stoppage or hindrance to the work, at the same time that the testing should be kept well up to the work.

After a great deal of consideration, and the abandonment of several plans, a system was devised which it is believed will be found the best for adoption in all future underground work.

It was found to be utterly impossible to keep testing the cables singly, and also manage to test the joints. The cables were tested in sections, and immediately afterwards all the joints of that section. The peculiarity of this plan is in so dividing the cables into sections (varying the length according to circumstances) and in so looping the wires at the two ends that a continuous circuit is formed. Any wire, therefore, in a joint-box will at once present the two ends of this continuous circuit, and render any tests of whatever kind perfectly easy.

By a reference to the diagram it will be seen that the wires represent a continuous circuit. A current being passed in at No. 1 would traverse backwards and forwards along the route, going through each wire, and finally returning through the last wire to No. 1 again.

The difficulty in joint testing with a wire extended is, that you cannot get at the end for "charging" the wire and at the joint for "observing" the leakage. In a cable factory you have the cable convenient and can get either end. Joints might be tested along a route by permanently charging from one end, but then you are helpless as to changes; it might also be done with two vans and apparatus, with one wire used for signalling, but that again would be expensive. In the plan adopted, instead of attempting to get to the distant end I brought it to me, and therefore had both ends of the cable and all the joints under my command as convenient as at a factory.

It will be seen that (referring to diagram), if it were necessary to test any set of joints in any one box, all that would be required would be to bring the van and apparatus to the box, then cut the temporary joint in No. 1, and you would have the two ends of a long wire; charge this to whatever potential required, each portion (the extreme end being free) would rise to the same potential, and to test any joint it would be only necessary to take it up and place it in the insulated trough, when the accumulation might be observed and its quality decided.

By means of an arrangement similar to this, every joint (save the last in the final splice of a multiple submarine cable might be tested.

The various jointers had instructions to make all the joints in the several boxes, with the exception of that in No. 1 wire, the two ends of which were left sealed or temporarily jointed. At stated intervals a box was left unjointed, thus forming a section. At this box the wires were looped in a certain order, 1 to 2, 3 to 4, &c. At the opposite end the wires were also looped, but in the opposite order, 2 to 3, 4 to 5, &c., so that the section represented a continuous length of the 14 wires; broken, however, at each box by the No. 1 wire not being jointed.

The following diagram explains this more fully :—

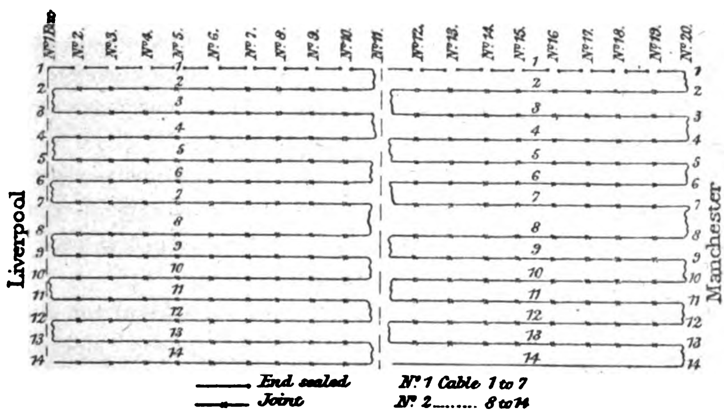


DIAGRAM OF ARRANGEMENT OF SECTIONS FOR JOINT TESTING.

When the various joints of a section had been finished, one jointer looped the last box (No. 11 in diagram) in the manner shown, according to instructions. The van then proceeded to No. 1 box, and careful tests of the copper resistance and insulation of each loop were taken; previously, however, currents were sent through to see that the wires were both correctly numbered and correctly looped. After finding that all was correct, the wires were looped at No. 1 box in the order shown, which gave a continuous circuit through all the wires with the exception of No. 1.

At the commencement of the tests, for a few boxes only, the joints were tried by seeing what insulation they gave. First, the joints being placed in water not connected to earth but perfectly insulated, the deflection due

to the leakage of the cable itself was taken, then the water was put to earth, the deflection was again noticed to see if there was any increase, such increase being evidently due to the joints only.

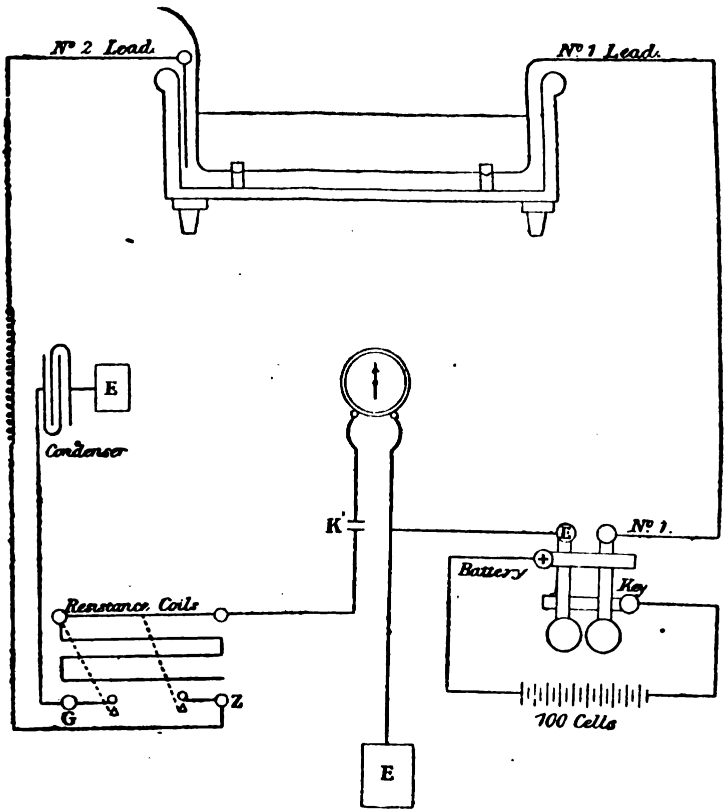
The instrument used was the Thomson Reflecting Galvanometer, fixed on a tripod; holes were cut in the bottom of the van, and the tripod rested on the ground without touching the van. Under these circumstances the Thomson acted very well, the only movements of the magnet being caused by the vibration of the ground, or the strength of the wind on the legs of the stand. With regard to the vibration of the ground, it was remarked that in those portions of the road bordering on the Old Mosses near Manchester, and at one time forming, no doubt, a portion of the moss itself, the vibration was peculiarly distinct, the presence of carts some hundreds of yards off being easily recognised, the vibration increasing with their nearer approach, until it became so great as to be no longer observable, being too much for the instrument.

Finding that observations could be taken with so much care and so much correctness, the "differential" method of testing joints was abandoned for that of "accumulation," the only correct and reliable method. By this plan any bad joints were at once detected. The delicacy of the instruments employed was very much lower than those in use for testing submarine cables, the ratio being as 1 to 10.

The *modus operandi* of joint-testing was as follows: When the van was brought up to the box it was placed as nearly as possible E. and W. (on account of the galvanometer not being astatic) and the horse taken out. Whilst the instruments were being adjusted, connections made, &c., the assistant and the jointer were engaged in opening the box and preparing the joints, whilst the driver was fixing the earth-rod and getting the different leads ready.

The joints were prepared by opening out and unlaying the tape back as far as necessary, the wires were then carefully and thoroughly cleaned with naphtha oneach side of the joint. The joint-trough was then cleaned and its insulation made good; it was partly filled with water and then placed in or over the underground box, according as the length of wire would permit. The joints were then put in the water and secured under the hooks, the cables being kept back from touching the trough by fastening back their tapes. One lead was then connected on to No. 1 wire, previously carefully trimmed, the second lead being joined on to the condenser, whose other end was attached to the accumulating plate in the trough. The third lead was joined on to the earth-rod.

The accompanying diagram will explain the internal connections of the van.



No. 1 lead was connected on the battery key, to which the two poles of the battery and earth were connected, the depression of the right hand part sending a zinc current (—), the left hand sending a copper current (+).

No. 2 lead was connected on to the battery key of the resistance coils, which acted as a switch, the left-hand or galvanometer key being connected on to the condenser (the opposite plates of which were to earth), the bridge portion of the coils, being in connection with the two keys (when depressed), was joined to the galvanometer, whose other pole was to earth.

A depression of the key z in resistance coils would at once place the

trough in connection with galvanometer to earth, any deflection (+) would at once show that there was leakage in the trough or over the cable. This had to be remedied before the tests could be taken. If no deflection was observed the tests were proceeded with.

A reference to the diagram, page 383, will show that when a current is passed into the cable through No. 1 it passes into 14, and so on throughout, until it comes again to No. 1 at No. 10 box, where the wire is sealed. The whole cable then is raised to the potential of the testing battery and the joints as well. If the joints be faulty they must leak, and some of the current must be continually passing into the water.

In testing, the key z is depressed so as to put the trough to earth through the galvanometer; the battery key is then depressed and the cable charged; the inductive effect on the trough is at once seen on the galvanometer by its sudden deflection. The cable is still kept charged, and after the inductive effect is over the "scale" is watched to see if the spot remains at zero, or is deflected in the same direction as the inductive charge. If there should be a deflection then there is what is termed direct leakage through the joint; if not, then the amount of accumulation is tried. The galvanometer circuit is then broken at K^1, and both c and z keys of resistance-coils depressed for a definite period—60 seconds; the leakage from the joints, however small, passes then into the condenser, where it is accumulated; at the end of the 60 seconds the key z is lifted and the condenser charge is passed through the galvanometer to earth. According to the amount of the discharge, so is the quality of the joints.

If the discharge was too great, or there was a direct loss through the joints, the joints were taken out of the trough and placed in one at a time in regular order; the discharges from each were observed and noted, the bad joints being marked for removal.

It is a peculiarity in testing a number of joints so placed that the accumulation obtained from the whole of the joints is always less than the sum of the accumulation of the individual joints.

The van containing all the batteries became, with one pole of the battery to earth, to a certain extent electrified. This was particularly noticed with regard to the condenser, which almost always had some residual charges in it, evidently due to being so close to the batteries. The effect of this was to introduce errors into the joint testing. This was noticed about the first few days of testing, and, after some experiments, the use of the condenser had to be abandoned altogether. Instead of it a long India-rubber lead was substituted, which, for facility of work, was wound

on a temporary wooden drum. This lead was found to act quite sufficiently, and was used to the last.

On the completion of the tests the joints were replaced in the box and the hole filled up, instruments and everything repacked, and the van moved on to the next box for further joint-testing.

The second box was tested in a similar manner, and so on, until the last of the section was completed, No. 1 wire being jointed in every case, generally permanently; but, where there were a number of bad joints, this wire was only jointed temporarily, so as to keep up the connection.

As soon as the jointer had replaced the condemned joints, his joints were tested (in the majority of cases), and the van proceeded to the loop-box of the section for the final test of that section. When the results obtained were satisfactory, the next section was taken, proceeding in a similar manner, and so on, section after section.

As a proof of the value and of the improved insulation obtained by cutting out the bad joints, two statements are given of the insulation resistance of the loops of two sections before and after the joints were cut out. In these sections *every* joint was condemned and cut out.

In the two cases the means of the 14 wires were—

	Before joints were cut out.	After joints cut out.
No. 1	111 megohms	481 megohms
„ 2	110 „	480 „

After the sections had been passed, one section was permanently joined on to the next, and so on; but no section was joined on until the previous lengths had been found to be still in good condition. It will be seen, therefore, that, as a frequent test of the passed work was going on, the appearance of any fault would be at once noticed.

I have been informed that the old Electric Company's "Express" wires had been struck through the ground by lightning. During the work the actual effect of lightning was not seen, beyond one man being incapacitated for a few days by being struck, but the *inductive* effects of lightning were brought to my knowledge by the van-jointer, who, jointing one day at a certain box, was alarmed by seeing sparks passing between the jointed wires (not covered with percha) and his joint box, against which they were resting.

With the present wires the boxes are all underground, except the street boxes. In the express wires the tops of the boxes were above ground.

On the completion of the work some final electrical tests were taken. The insulation tests were taken in the ordinary manner, the deflection being noted at the end of the first minute. From the deflection the calculations were made. The deflection due to the leakage was carefully watched from the commencement, and in every case of the gutta percha tests, except No. 3 Liverpool, the "electrification" was evident and satisfactory; the insulation and per-centage of loss being taken as in a cable factory.

The copper resistance was taken by carefully looping the wires together at the opposite end, using a metallic circuit only, doing away entirely with all earth connections and such sources of errors.

First, No. 1 was joined to No. 2,
No. 1 then joined to No. 3,
No. 2 „ to No. 3.

From these figures the resistance of No. 1 wire was calculated by formula. No. 1 was then used in connection with all the other wires.

The formula is thus expressed. (W. H. Preece)—

Call No. 1 wire $= x$
No. 2 „ $= y$
No. 3 „ $= z$

Then—

$$\left.\begin{aligned} x + y &= a \\ x + z &= b \\ y + z &= c \end{aligned}\right\}$$ These are known by direct measurement.

Subtract 1 from 2,

Then $y - z = a - b$

But $y + z = c$

$$2\,y = a - b + c$$

$$y = \frac{a - b + c}{2}$$

$$x = \frac{a + b - c}{2}$$

$$z = \frac{b + c - a}{2}$$

The resistance of No. 1 wire will be, in other words—

No. 1 $= (1 + 2) + (1 - 3) - (2 + 3) \div 2.$

The R. of 1 and 2 + the R. of 1 and 3 — the R. of 2 and 3 divided by 2, gives the required R.

This method, wherever practicable, is far superior to the use of an earth, which necessitates duplicate tests.

TESTS OF GUTTA PERCHA CABLES,

Reduced to 75° Fah.

	No. of wire.	Copper Resistance.		Insulation Resistance.	
		Observed per stat. mile.	Red. to 75 per stat. mile.	Observed per stat. mile.	Red. to 75 per stat. mile.
		Ohms.	Ohms.	Megs.	Megs.
	1	25.16	26.51	351.5	55.9
	2	25.13	26.48	540	85.8
	3	25.10	26.44	337	53.6
	4	25.24	26.59	595	94.6
	5	25.21	26.56	584.5	93.0
	6	25.24	26.59	496.5	79.0
	7	24.88	26.21	838.0	133.3
	8	25.13	26.48	486	77.3
	9	25.19	26.54	576.5	91.7
	10	25.10	26.44	493.5	78.5
	11	25.10	26.44	453.5	72.1
	12	25.06	26.40	533.5	84.8
	13	25.16	26.51	580	92.3
	14	24.91	26.24	659	104.9
	Mean ...	25.116	26.46	538	85.6
Wharf Road ...	Mean ...	—	26.23	—	483.0

The copper resistance given is inclusive of the India-rubber-covered wires in Warrington; the gutta percha resistances are the resistances of the gutta percha wires alone, exclusive of India-rubber cables.

STATEMENT OF FINAL TESTS OF THE GUTTA PERCHA CABLES.

No. of wire.	Insulation Resistance. Warrington to Liverpool.	Insulation Resistance. Warrington to Manchester.	Mean Ins. Res. Liverpool to Manchester.	Copper Res. Liverpool to Manchester.
	Megs.	Megs.	Megs.	Ohms.
1	310	393	351.5	25.16
2	495	585	540	25.13
3	117	557	337	25.10
4	594	596	595	25.24
5	594	575	584.5	25.21
6	565	428	496.5	25.24
7	626	1050	838.0	24.88
8	387	585	486.0	25.13
9	557	596	576.5	25.19
10	457	580	493.4	25.10
11	350	557	453.5	25.10
12	510	557	533.5	25.06
13	575	585	580	25.16
14	523	795	659	24.91
Mean ...	477	599	538	25.116
Mean of 2 centres	574.5	922.5	748.5	24.896
Mean of 12 outsides	459	545	502.0	25.15

The results are reduced from the temperature of 50° Fah., which is calculated from the observed copper resistance, the length taken being that of the actual route, plus allowance for leading into offices.

The standard for the temperature being the mean results of the tests of the whole of the core taken at Wharf Road.

From a comparison of the final tests made and reduced to 75° with those taken of the core at Wharf Road, it was found that the difference was very great, the insulation having fallen a large amount. This is

due to several causes, the principal being the action of Stockholm tar on the gutta percha, rendering it, so to say, slightly porous and reducing its resistance. This has been noticed for some time, but the following experiments show in a marked degree the large reduction in insulation caused by the use of Stockholm tar.

The other causes which tended to the reduction of the insulation were "tension," due to hauling in the cable, and "friction" against the sides of the pipes; the latter was found to operate the most strongly.

It might be asked, if tar reduced the insulation of gutta percha so much, why use it? But it must be borne in mind that it is only in water that gutta percha has been proved to be lasting, elsewhere it rapidly perishes without some protection. Stockholm tar has been found to be the best of these preservatives, and its advantages may be safely said to outweigh its disadvantages.

Some very strong evidence in favour of the use of tar has been proved almost everywhere, and notably with the connections between the Submarine Company's land wires and cables in France.

The following experimental tests show the rapid fall of insulation which took place with some coils of wire in a factory:—

EXPERIMENTAL TESTS to see the action of Tar on Gutta-Percha-covered Wire No. 18, Copper, to No. 7 G.P., two lengths of a mile each.

11th October, 1872.

TEST BEFORE BEING TARRED.

Copper Resistance.

No. of coil.	C.R. obt.	Lead.	C.R. per mile.	Temp.	Red. to 75.
1	26·06	1·12	24·94	54°	26·043
2	25·28	1·12	24·16	54°	25·228.

Capacity, 10 Cells.

No. of coil.	After 1′.	Immediate.	Per cent. loss.	Cap.
1	222 × 10	240 × 10	7·288	·27146
2	221 × 10	239 × 10	7·498	·27026
Lead	125	136.		

Dielectric Resistance.

No. of coil.	After 1′.	Resistance.	Reduced 75.
1	34	3583·7	702·96
2	33	3692·3	724·26

Constant $= 12185 \times 10^6$

October 23rd.

TESTS on same COILS, after being Tarred and Taped *a week*, and then put in water 18 hours before Testing.

Copper Resistance.

No. of coil.	Copper Resistance.	Temperature.
1	25·35	54°
2	24·45	54°

Capacity.

No.	Per cent. in 1′.	Capacity.
1	10·18	·27649
2	10·26	·27428

Dielectric Resistance.

No.	After 1′.	Resistance.
1	51	2577
2	50	2628.

Constant, &c. $= 13142 \times 10^6$.

1*st November*, 1872.

Copper Resistance.

No	C.R. per Mile.	Temperature.
1	25·44	56°
2	24·54	56°

Capacity.

1	13·288	·27494
2	12·485	·27494.

Dielectric Resistance.

1	1979·1
2	1949·5

Constant $= 13062 \times 10^6$.

7*th November*, 1872.

Copper Resistance not taken. Temperature 56½°.

Per cent. Loss.

1	13·288
2	14·393.

Dielectric Resistance.

1	1832·6
2	1759·4.

Constant 13195×10^6

5th March, 1873.

Copper Resistance.

1	25·47	56°
2	24·59	56°

Capacity.

1	12·174	Cap. ·27578
2	14·655	·27818

Dielectric.

1	1644·2
2	1118·1.

Constant 27951 × 10^6.

Gutta Percha Jointing.

Of the various operations in connection with practical telegraphy, there is scarcely one so important, or requiring so much practice and experience, as the making a joint in a gutta-percha-covered wire, and unfortunately t is an operation that in telegraph companies had received but little or no attention, and became the greatest difficulty I had to contend with throughout the work. Joints had to be condemned wholesale, and the causes were found to be, in having men not good enough for the work, some temporarily physically incapacitated, and in others minor faults. Of the original number of joints made, about 34 per cent. were condemned and had to be replaced.

The two most extraordinary instances noticed were, first, the jointer, who had been jointing from nearly the commencement of the work until all the wires had been drawn in. His joints looked well and tested well, here and there meeting with a bad one ; this went on for some time, until his joints appeared, for no particular reason, to go bad altogether, necessitating their entire condemnation.

From Broad Green to a place about four miles beyond Warrington, this man's joints were gŏod ; from that point to the last of his joints they were almost entirely bad, and for miles every joint made by him had to be cut out and remade.

The second case is more remarkable. Being scarce of jointers (having removed three), a special one was sent down from London. The first set of joints he made were tested very shortly after making, and they were found

to test perfectly; he was then set on to joint regularly. Subsequently all his joints were bad; he had got unwell, and was physically incapacitated.

I would now call attention to several sources tending towards inferior joints, and also what are usually the causes of bad joints.

Dissimilarity of Material.

This may be termed a very frequent cause of inferior jointing and one in which no blame can be attached to the jointer himself, as he has to work with the material supplied to him. Care, therefore, should always be taken that proper material is served out.

In one submarine cable I had charge of, the joints would not test properly; some time after they were made they invariably went bad, and from bad to worse. The same result attended the productions of several jointers, all of whom were rated as excellent men. Having, however, had various joints made with various kinds of sheeting, in testing these joints I came upon those that did test well; and on examination it was proved conclusively that a wrong material had been previously used, preventing a perfectly homogeneous joint.

Physical Reasons.

It is unquestionably the fact that some men are prevented from making good joints in consequence of physical causes. A greasy sweat issues out of the pores of the hand, and, as a joint requires frequent touching, it is easy to understand that this affects the material and will prevent a proper junction between the coatings.

It is also a fact that by reason of ill-health an experienced jointer is similarly affected and makes inferior joints.

Weather.

In out-of-door jointing the inclemency of the weather frequently interferes with good jointing. The difficulty of keeping the lamp alight, and the difficulty also of applying even heat, are deleterious. Bad weather is disagreeable. "A wet jacket is uncomfortable." "Very," the jointer says, and finishes as quick as he can—certainly not to the improvement of his joint.

Cleanliness.

The absence of cleanliness is one of the most serious defects a jointer can possibly suffer from, and one which is the cause of more bad joints

than anything else. Too much attention cannot be paid to keep the wires properly clean, the material and tools clean, and the hands as well.

The faults that beginners usually suffer from are simply the results of a want of experience which practice alone can get over. They all suffer from it, and in course of time, provided they have sufficient practice, they all get over it.

The principal of these may be simply stated as follows:—

Bad twist in copper.
Ends sticking up.
Bad soldering.
Nicks in copper from trimming carelessly.
Wire and joint out of centre of core.
Gutta percha joint away from copper joint.
Air-holes.
Overheating and burning.
Imperfect junctions and seams.
Compound peeling off.
Damage to core—bad handling.
Cutting off sheeting too close.

I now give a more detailed explanation of the manipulation rendered necessary in making a joint in No. 7 than I think has ever appeared before; it includes some valuable hints. It has been written as the result of seeing an innumerable quantity of joints made, and, from minute particulars given by the most experienced jointers, I believe it will be found valuable.

Instructions for Out-door Jointing No. 7 Wire.

Preparatory.—The joint-box where the joints are to be made is first opened, the jointer's box placed close to one side of it, and then the tent placed over the box so that the opening in the tent is opposite the jointer's box.

Attached to the box should be two low stools for the jointer and his assistant to sit on to keep them clear from the wet pavement or damp ground.

The box should be opened, and the various tools, spirit-lamps, furnace, &c. placed where most handy; the spirit-lamp for the furnace should be lighted and the soldering-iron heated; the gutta percha tools should, if dirty or sticky with compound, be filed and cleaned.

Great care must be taken to keep the gutta percha sheeting perfectly clean and dry.

The wires leading in one direction are then taken out and prepared for jointing.

Cleansing Wires and Numbers.—If they are in a multiple cable, by stripping off the tape about 15 inches back and fastening round the cable, loosening the numbers and passing them down the wire to the tape (great care must be used in passing the numbers down, for unless they are well loose they will damage the percha); when the number is passed down to the tape it should be fixed there. When each wire has been served this way the whole of them should be cut to exactly the same length.

The same plan should be adopted with single wires.

When the above is done at one side, the jointer should do the same at the other side.

The dirty work ought properly to be done by an assistant.

Cleansing Wires.—The wires at both sides must then be thoroughly cleaned with white waste soaked in naphtha, until each wire becomes thoroughly clean, free from tar, dirt, and grease.

Cleansing Hands.—After cleaning the wires the jointer should very carefully clean his own hands, and dry them well. Naphtha will be found best for this purpose. Its disadvantage is that it has a tendency to harden the hand.

The wires are then ready for jointing.

Trimming Ends.—No. 1 wire should then be taken up on both sides (it is best to begin with the lowest number and proceed in regular order) and the gutta percha carefully trimmed off each end for about 1½ inch, care being taken that the knife does not "nick" the copper; if this should happen, the copper must be cut off at the "nick" and the percha trimmed back.

Making Copper Joint.—The copper wire left bare should be scraped carefully, and then, the two ends being brought together overlapping, may be held by the pliers, and first one side twisted then the other; the entire twist should occupy about 3 turns each way, or ¾ inch; the surplus ends should then be cut carefully and close over, being lightly touched with the pliers turned in, so as not to leave an edge sticking up.

Soldering.—The twisted joints should then be soldered, care being taken to knock off superfluous solder. Great care must also be taken, when soldering a joint, that no wires be immediately under it, but that

the space underneath be quite clear. Hot solder dropping on gutta percha at once heats it and penetrates.

Corresponding Numbers.—The remainder of the wires should then be jointed and soldered; great care must be exercised in jointing similar wires; the jointer should himself see that the numbers correspond, and not trust to his man giving him the numbers without himself seeing that the numbers are correct.

The gutta percha jointing may then be commenced, and the second spirit-lamp lit for warming the material; care, however, must always be taken that the spirit-lamps and furnace are so placed that they cannot injure the gutta percha.

Clean Joint.—The ends and soldered joint should first be cleaned with naphtha.

Compound.—Then a stick of compound should be warmed, and a small quantity put on the copper and joint, and properly tooled over, so as to cover the joint equally. Before applying the tooling-iron it should be well wiped.

First Cover.—The ends of the gutta percha are then slightly warmed and the actual ends nipped off with the fingers. One side of the percha should be well warmed for about 2 inches back, and then brought forward over the joint to the opposite end with a twisting motion; the end should be nipped off, the opposite end after heating should then be brought forward over the other part in a similar manner, as far as it will go; the percha should again be warmed and kneaded together with the finger and thumb, or tooled beforehand.

Compound.—After kneading it should be warmed over slightly with the spirit-lamp, the compound should then be heated and applied over the gutta percha, not by *dabbing*, but by putting the stick on the percha and *rolling* it along sufficient will be found to adhere; the compound must be again warmed and applied a sufficient number of times to go thoroughly over the percha.

The joint should again be warmed and the compound properly tooled until it covers the joint uniformly.

Second Cover.—A sheet of gutta percha (the gutta percha sheeting, as supplied to jointers, should be cut into strips 4in. long, and kept carefully in a bag or case), well cleaned, should then be warmed carefully over the spirit-lamp, and, when sufficiently warmed, a piece of about 1in. should be cut off with a pair of scissors, whose edges are moistened with the lips (the pieces cut off should be put in the mouth to assist formation of saliva), the ends should also be cut off; the joint

is then warmed with the lamp, and also the piece of sheet that is held in the hand, previously having been stretched ; the sheet is then applied to one end of the joint, ½in. on the old core from each end of the pull down, and, being firmly pressed, is drawn along the length of the joint ; the superfluous end being cut off, the joint is then turned over, and the spirit lamp applied so that the flame warms both joint and sheeting ; the sheeting is then pinched round the joint so that its sides meet above the joint ; the upper part is also slightly pulled so as to make the adhesion better ; the spare sheeting is then cut off with the scissors close to the joint, a warm tool is passed over the seam so as to open it again, it is again pinched up, and by so doing forces out any air that may be in it. In pinching up the last time, one edge ought to overlap the other slightly, so that the warm tool may more properly seal up the seam.

By cutting off the sheet too far from the joint the seam cannot be re-opened, and by cutting off too close no seam is left, and there is necessarily a vacant space in the second covering ; this is a frequent fault, and should be avoided. By use of the tool the ends of the coating are made to amalgamate with the old material, the joint is again warmed thoroughly, and kneaded with the thumb and forefinger, care being taken to preserve its shape and to knead evenly all round ; it is then rubbed up with the moistened hand.

Compound.—The stick of compound and the joint are again warmed, and the compound is rolled over the joint from end to end in about four places, which about covers the joint ; the joint is again warmed, and the compound is worked and spread over the whole joint, by means of the tooling-iron, in a uniform and even manner. The joint is again manipulated with the hand, and kneaded. It is then heated for the last time, and rubbed well with the hand well moistened. This rubbing must be done uniformly and equally all round ; it tends to solidify the joint, and gives it that highly-polished and finished appearance so characteristic of the handiwork of a good jointer.

The following notes respecting the joint and its manipulation should be carefully attended to.

Holding Core.—The jointer and his assistant should hold the wire carefully and firmly between the thumb and forefinger at such a distance from the joint as to be beyond the influence of the heat ; the percha held should always be hard ; if the hand be too near the joint the man will probably be pressing the material where it has been softened by the heat, and will very probably cause damage.

Twisting Joints.—In turning the joint over for the purpose of heating or tooling, the jointer and his assistant should turn it over carefully together so as not to put a twist in the short portion, but to distribute it over the entire length. When that operation has been done the joint should be turned back in the opposite direction, so as to bring it into its original position. As in making a joint this twisting has to be done very often, it is very essential that this turning over and back again should be attended to, instead of turning the joint the same way always; from inattention to this and the preceding instruction, joints have been seen with a series of twists made outside the joint.

A jointer should bear in mind that a good jointer is known by the unaltered state of the core outside the joint, as well as by the excellence of the joint itself.

The application of the fingers to the joint is frequently necessary; the fingers, however, should be well moistened before touching the warm material.

Whenever a joint has been touched by the moistened finger the joint should *always* be warmed with the spirit lamp, as this drives away any moisture. *This is very important.*

If in warm weather the hand should perspire it ought to be dried. Naphtha will do this best, especially as its rapid evaporation produces coldness.

The length of a joint should be about *six* inches.

The principal faults in joints are :—

Bad twists.—Nicks in copper and leaving *one* end of twist sticking up; joints have been seen with one end nearly penetrating through.

Eccentricity.—Due to bad kneading and tooling.

Air-holes.—Due to bad closing of sheeting.

Burning.—Due to careless use of lamp and overheating.

Separation of coatings.—Due to moisture remaining on coats and also to want of *cleanliness.*

Every detail of jointing may be learnt, and with practice become quite easy; but, unless a jointer is scrupulously clean, his labour is nearly useless.

Cleanliness is of all things the most important.

Sheeting of a similar quality to the gutta percha of the wire itself should always be used. It is very difficult to produce a homogeneous joint by using two different materials.

The preceding details give the most important points with regard to this work, and explain as much as possible those points where special care was taken. It was in the testing department that this was most attended to, and the fact that the extreme delicacy of testing as usually applied to submarine cables was in a less sensitive degree applied also for the first time to a work on land, will show that as regards electrical points no stone was left unturned to facilitate and insure success; each wire was constantly tested; every joint underwent a searching investigation; and the result of the whole operation was a work of electrical success, which time alone can show to be permanently lasting.

APPENDIX.

Decay of Gutta Percha.

The following observations on the state of the underground wires of the British Electric Telegraph Company are given from two reports of Mr. Edward Highton to the Directors of that Company :—

Having understood that the wires south of Berkhampstead had failed in many parts, I went there yesterday,* with a view of endeavouring to ascertain the cause of such failure. I selected for examination a district commencing about a mile to the south of Berkhampstead. I selected a length where the wires passed near the roots of oak-trees, and then near the roots of ash and Italian poplars, with only one oak-tree among them. I found the wires and wooden boxing had failed, and had been renewed for several yards in passing every single oak tree, including the isolated one above mentioned, and nowhere else.

I had the earth removed from the wires at various places, and selected, in particular, those spots where the newly replaced wires and boxes joined the old wires and boxes. I found the boxing laid down in March last in a state of decomposition—whilst old boxing, put down some two or three years ago, and within seven yards of the same, was perfect. I have found wires perfectly good and completely rotten within seven yards of each other. This proved the action to be local. My attention was then directed to the probable cause of the decay.

On opening the first part where the wires were decayed, I observed a remarkable peculiarity in the soil; I detected, at once, a whitish-looking plant, resembling the spawn of the mushroom, or some other fungus,

* September, 1856.

pervading the soil and filling every crevice; I found that it had utterly destroyed all the dead roots of the oak and plants in the edge. Its branches spread all over and around the wooden trough, covering it with a whiteness resembling a whitewash. I found wherever the plant touched the gutta percha wires the gutta percha was rotten. I find that the wooden troughs laid down in March last in the vicinity of this plant are more rotten than troughs within seven yards of the same (where is no trace of the plant), which have been down since the commencement. I anticipate the whole of the wires which have lately been laid down in those particular parts will again decay in a short period of time. The breakage that must have taken place, and which is taking place, in spots over a length not exceeding one-third of a mile, is quite enough to stop all telegraphing between Manchester and London.

On my first noticing this peculiar subterraneous plant, I thought it might be the spawn of a certain fungus. I immediately searched for funguses under those oak-trees; I found a yellowish green fungus luxuriantly growing under every single oak without exception, but not one under an ash or other tree.

Whether the vegetable production I found be the spawn of that fungus or not, I cannot say. The facts observed to-day would almost warrant that conclusion.

But, as a botanist, my impression at present is, that it is not so, but that it is an entirely different plant; whatever the plant may be, I cannot believe that any vegetable product, destitute of organic life, can resist its decomposing action.

The plant possesses a powerful odour: after breaking soil a few inches deep, its scent can at once be detected. The presence of the plant and the decay of the wires I found coincident. The absence of the plant and a *most* perfect state of the wires, coincident also. Such are the results of my examinations of yesterday. With the permission of the Board I intend to pursue these investigations.

I now beg to forward a sample of the fungus growing under every oak-tree where the wires have failed. I send also a sample of the earth with the decomposing plant therein; its peculiar scent will at once be apparent. I send also a specimen of the decayed boxing in the vicinity of that plant; and I also send a portion of a root acted upon by that plant.

In conclusion, I beg to say all the wires examined by me yesterday were fully two feet deep.

Since my last report I have carefully examined the spawn-like appearance which I found in the ground surrounding the wires near Berkhampstead.

I have applied to it microscopic power up to 500,000.

The substance presents all the characteristics of the mycellium of a fungus, but of what particular fungus, by name, I have been unable to ascertain.

I have arranged, however, with the Royal Horticultural Society to have the case, with its concomitant facts, submitted through that Society to the most eminent authority on fungi in Europe.

I have again examined the wires near Berkhampstead. I found the wires passing under two oak-trees at a particular spot had in no way been affected. I could find no trace of the fruit of a fungus in the soil around those two trees; around every other oak-tree in the neighbourhood, under which the wires had decayed, I found abundance of the fruit of fungi, while around the two trees above-mentioned, although in the same neighbourhood, there was no fruit at all to be seen, and I was informed by the men who had opened the ground that there had not been any of the spawn-like appearance found there.

On Tuesday last I examined the wires between Warwick and Birmingham; my attention was first directed to a place where the wires passed under a very large oak-tree, the stem being 16 feet in circumference at 3 feet from the ground, and the branches extending 45 feet from the stem. The tree is known in the neighbourhood as Old Parr's Oak. The wires under this tree have decayed and have been renewed.

I searched for the fruit of a fungus, and immediately found abundance of it, and apparently of the same kind as those under the trees near Berkhampstead where the wires had rotted. I then had the soil removed and examined the new wires.

I found the wooden lid of the testing box decayed, and covered with both dead and living mycellium of a fungus. I found on the gutta percha of the wires in the 2 feet which I opened two portions of the mycellium of a fungus, each in area about the size of a penny-piece, and also one under the lid of the test-box.

I have the evidence of two of the Company's workmen, and also of a person living near the spot, that the ground when opened for the renewal of the wires was filled with a substance resembling the spawn of a fungus. I learn from three different witnesses that the ground about a mile from Berkhampstead where there are no trees, but close under a hedge com-

posed of hawthorn, and a shrub commonly called dogwood, presented the same white-like appearance, and that the same appearance presented itself under every oak-tree where the wires had decayed.

There is, therefore, abundant evidence that where this spawn-like appearance is found in the ground both the gutta percha and the wooden boxing have decayed, while under similar circumstances the same kind of wires and boxes, when the spawn-like appearance was absent, had not decayed.

Believing, therefore, this whitish appearance to be the mycellium of a fungus, my next inquiry was, if so, what its effect would be upon vegetable matter, such as wood and gutta percha. I referred to a work published only a few weeks ago, and under the article now alluded to I find reference made to eleven English and foreign works in confirmation of the statement of the author.

The author says:—"The decay of wood is often greatly accelerated by the growth of the mycellium of fungi, which seems to decompose the organic compounds in the wood in the same manner that yeast does those in organic liquids. A general law prevails throughout the fungi, that their nutrition depends exclusively upon the absorption and decomposition of organic compounds, therefore consisting of the performance of the operation of fermentation upon the organic matters upon which they feed."

I am now, however, arranging a series of experiments, which after a short time will enable me to speak positively upon the point.

And next with reference to the decay of gutta percha in iron pipes—

I have as yet examined but two places, viz., one at Knowle, near Birmingham, the other at Winslow in Buckinghamshire; at Knowle I found the wires had been renewed, but, as the new ones were in good condition, I had little or no data for investigation; suffice it to say, I noticed there a remarkable peculiarity, which I found existing also at Winslow.

At Winslow I ascertained that there were only 46 yards of iron piping, the wires passing through the rest of the town being in wooden troughs. I found the old wires in a state of decay through the entire lengths of the iron pipings, with the exception of one inch at either end. The wires in the wooden boxing in the immediate neighbourhood, and up to the commencement of the iron pipes, were as perfect as they were when first laid down.

In one wire at Winslow the gutta percha was so decayed and cracked that the internal copper wire was visible.

The decay of gutta percha in iron pipes appears at present to me to be produced by a cause entirely different from that under the oak-trees alluded to. And, although I have already formed my own opinion as to the cause of this decay, I would rather not express that opinion until I find it verified by further observations.

I also examined the wires near Solihull, which had replaced the old wires which had decayed. The soil in which the wires had decayed was a stratum of blue marl overlying red clay; but, as the new wires and boxing were perfect, I had no data for the investigation of the cause of the decay of the previous wires.

During my late experiments, I have not found any difficulty in completely destroying one of the most destructive fungi in Europe, without doing the slightest injury to the plant on which it was growing.

In conclusion, I would observe that there appears to be a number of isolated spots, of a few yards in length only, where the gutta percha has decayed, while at each end of those particular spots, and in the intervening parts, the wires are as perfect as one could possibly wish them to be. I believe all the cases I have examined are attributable to causes which may be obviated by further scientific investigation.

With reference to my experiments on the action of the mycellium of a fungus on gutta percha, I have for some months been growing one of the class called *agaricus campestris* in contact with gutta percha.

I find as the result that the mycellium of this fungus does rapidly destroy the insulating properties of gutta percha; and in fact it appears to decompose entirely this vegetable gum. I send a sample showing the decay.

I am trying further experiments, an account of which I hope ere long to lay before the Board.

A few days ago I examined a spot near Canterbury where the gutta percha of the wires had entirely decayed.

The soil was pure, clean, sharp, red sand, and there appeared nothing in such soil to induce decomposition.

But at that spot I found a young oak-tree, which could only derive its nourishment from the ground through which the wires passed; and upon these roots, both living and dead, I found what I believe to be the mycellium of a fungus, the same as that which I discovered under the oak-trees at Berkhampstead.

I send with this some of those roots upon which the fungus can be distinctly traced. The odour arising from that fungus appears to be identically the same as that from the fungi at Berkhampstead.

Ten yards distant was also another young oak-tree, and at that point the gutta percha of the wires had also decayed. I will again communicate with the Board when I have made further investigations, but at present I feel bound to say, that the presence of the mycellium of a fungus, and the decaying of the gutta percha covering of the telegraphic wire, being so constantly associated together, I can come to no other conclusion than this, viz.—that the mycellium of a fungus will cause decomposition in gutta percha and probably in most other vegetable productions.

July, 1857.

"TO THE DIRECTORS OF THE BRITISH TELEGRAPH COMPANY.

"Gentlemen,

"I beg to hand you several specimens of gutta-percha-covered wire, and also sheet gutta percha, which have been experimentally subjected to the action of the mycellium of a fungus, viz., the *agaricus campestris.*

"The gutta percha was placed in different parts of a bed of soil 5ft. 6in. wide by 5ft. 3in. long. The spawn of the fungus was placed in the soil, at intervals, over that space, in the month of September, 1857.

"The mycellium traversed the whole of the bed.

"The gutta percha sent has not been touched by the hand or a tool until exhumed on the 25th of January, 1858.

"The several specimens sent will show the complete destruction of gutta percha by the mycellium of a fungus, and prove, I trust, the correctness of the opinion I expressed many months ago.

"Should the Company desire any further experiments, I shall be happy to make them; but I consider those already made by me so conclusive as to render further experiments in the same line unnecessary.

"I am, gentlemen, your obedient servant,

"EDWARD HIGHTON.

"London, January 28th, 1858."

Mr. GEORGE SAWARD, in his evidence before the Submarine Telegraph Committee in 1861, remarked, that in an examination of the London and Manchester underground wires he found the gutta percha wires decayed, and universally the case wherever oak-trees or oaken posts drained on to the line. A remarkable instance occurred in the Edgware Road, where a series of posts and rails were placed along the side of the road to protect the

footpath, which is higher than the other portion of the road. The wires were laid in the lower portion, and it being a gravelly soil any drainage trickled down in the direction of these wires. More than a mile of the wire was taken up, and there was the mark of every post along the gutta-percha where it was taken up; wherever there was drainage from an oak post, there was a piece of decay.

In the vicinity of oak-trees, or even of oaken posts, where the wires lay so that the drainage from the oak trickled down to the boxes containing them, and also in cases where, for economy, the trenches had been dug in the soft hedge bottom, and in other situations favourable to the growth of fungi, it came up when fresh in a soft pulpy state, and in many instances in such a state that the gutta percha might be wiped off the copper with the finger.

Mr. W. T. Henley, before the same Committee, observed, that he had come across several instances where a white fungus had been formed round the wire, and at every place where it appeared the fungus was as white as a piece of paper, and the gutta percha cracked on bending; this effect was not observed in 40 miles of wire covered with tar, but only where the wires were naked and unprotected.

DISCUSSION ON THE PAPER.

The Chairman: Gentlemen, as there is to be a special meeting after this, at which we have to consider alterations in the Rules, the discussion must necessarily be curtailed at half-past 9. I will therefore not occupy time by any remarks of my own, but I shall be glad to hear the remarks of any gentleman. The paper is one which is sure to elicit a good discussion, and perhaps there is plenty of room for it. I see Mr. Graves present; will he favour us with a few observations?

Mr. Graves: Anything I could say would be to a great extent a re-echo of what we have heard from Mr. G. Preece this evening. Having seen the paper before it was read, perhaps I had better abstain from giving any opinion.

The Chairman: You have had experience on the Great Northern and North Eastern Lines as to the causes of failures; that I know personally.

Mr. Graves: Certainly; but perhaps the reasons for the failures were throughout one and the same. They arose simply from inattention to the conditions which Mr. G. Preece correctly describes as essential for the manufacture of a good joint. The joints were cut and were afterwards mended, without the slightest consideration of the conditions which are necessary to produce anything like a perfect result. Men attempted to unite the thick pieces of gutta percha by means of thin skins of the same material, or by simply bringing together two portions of material under the conflicting conditions stated, each at a different state of temperature, and consisting often of very different specimens of manufacture. We had of course in the tunnels to which Mr. Varley alludes many failures ten or fifteen years ago. They arose partly perhaps from primary defects in the material itself, but certainly more frequently from deficiency in the jointing. The jointing was carried out by rule of thumb. No process of test was applied, nor was any careful supervision brought into play. The ordinary line-men were supposed to be experienced in the union of sections of gutta percha, and they carried out the operation after this fashion: The two ends of copper wire were scraped; the scraping was by no means a perfect process, and very often you had irregular surfaces further separated by dirt. Then round the main copper wire they wound first a length of thinner wire as a binder, perhaps made firm by solder, perhaps without it; next strips of thin gutta percha; the length was indefinite, and the adhesion of the successive layers imperfect. Then they pulled the half-softened ends of the gutta percha together, and the whole thing was welded into and smoothed into form with a hot iron. Thus the joint was made. Save in cases of rare exception there was a total absence of care to prevent the adhesion of dirt; there was no thought with reference to the effects that might be produced by moisture, either of the atmosphere or of the hands, and no inquiry into the requisite conditions for a perfect union of all the materials. The result was that you had something outside looking perfect outside, but which with every successive month deteriorated. No gutta percha wires laid down more than ten years ago remained perfect, except by mere accident, and none of them, so far at least as my knowledge extends, can show any pretence to good work carried out so as to secure good results. The fact is it is only within the last three or four years that the conditions stated by Mr. G. Preece have been appreciated, and the instance of which he details the history is the first I have heard of in which anything like scientific care was used. There has been a very large quantity of gutta

percha wire laid in pipes, in streets, and on roads, and the cause of the frequent failures there can be no doubt rests in carelessness in putting the parts together; but the great and dominant reason, which extends throughout, is the general ignorance prevailing of the conditions necessary to a perfect combination of materials, and it is only by careful action upon the principles necessary to secure something like a perfect union that there is any hope of obtaining success. If the experience derived from the underground cables to which Mr. G. Preece alludes holds good for the future as it has at present, the care which has been taken certainly justifies itself by the results. Of course the time (little more than two years) which has elapsed since this line was completed does not warrant an inquiry much, but at all events it warrants us in stating this much—that no underground line that has ever been yet laid has after the lapse of the same period shown anything like the same high tests. Mr. G. Preece has given us the tests that presented themselves after the completion of the work. I will merely say that with the exception of one particular experimental length, which was laid down under special conditions that do not affect the general bearing of the work, there has been scarcely any change whatever in the condition of the cable during the last eighteen months, and it now stands higher than any similar gutta-percha work of any length with which I have acquaintance.

Mr. R. Fischer von Treuenfeld said: It may be interesting to mention a few facts with regard to the early history of underground telegraph wires. The first underground wire was laid as early as 1842 by Professor Jacobi, in St. Petersburg, Russia, which consisted of a copper wire covered with cotton, and varnished, and laid in glass tubes. In 1845 the first samples of gutta percha came from India to Europe, and Dr. Werner Siemens made a series of experiments with gutta percha with the view to use the new dielectric material for galvano-plastic purposes. In 1846 the learned doctor used for the first time gutta percha for covering telegraph wires, and also used such insulated wires for underground telegraph lines. After these first experiments had established the satisfactory result to furnish good underground wires, the Prussian Government Telegraph Administration decided in 1847 to have this new system of telegraph wires introduced on an extensive scale, and they in fact laid about 1200 miles of gutta-percha-covered underground wires, which Dr. Siemens in connection with two then well-known india-rubber manufacturers—Messrs. Fonrobert and Pruckner—manufactured in Berlin. Although the Prussian Telegraph Administration was at that

time and, I firmly believe is still, of the opinion that underground wires are the most recommendable wires for telegraph lines, nevertheless their expensive experiment in 1847 turned out to be a failure. The reasons for this are obvious. The system was sound and recommendable, but the experience and knowledge how to manufacture gutta-percha-covered wires, so that they possess the required insulation and all the mechanical qualities of a good cable core, were at their very infancy of cable manufacturing by far too imperfect. The Prussian underground telegraph lines failed because they did not and they could not insulate; but they had one immense and valuable effect, namely, to be the first and most important step in our present submarine telegraphy. They introduced gutta percha as an insulating material, and furnished very valuable results with regard to its use and manufacture.

Major Webber, R.E.: As Mr. G. Preece's paper is actually very much longer than the portion he has read out, and as it is probable it will elicit a good many remarks from other members at the next meeting, it may be worth suggesting that the paper should be printed, if there is time between this and the next meeting, and circulated to such members as wish to see a copy, in order to prevent any member in his remarks introducing statements which Mr. G. Preece has already made in his paper.

The Chairman: The paper will be printed and circulated throughout the Society.

The discussion was then adjourned.

The following Candidates were balloted for and declared duly elected:—

As Foreign Members:—

D. H. Bates	Philadelphia.
A. S. Brown	New York.
Albert B. Chandler	New York.
Frédéric Délarge	Brussels.
T. Dolan	New York.
J. H. Dwight	Chicago.
Joseph Girardin	Brussels.
George J. Ladd	San Francisco.
Dr. Ad. Lasard	Berlin.
J. Merrihew	Philadelphia.
G. F. Milliken	Boston, U.S

As Associates :—

F. R. Brown	Calcutta.
Lieut. J. Bucknill, R.E.	Whitehall.
F. H. Carpenter	Para.
Edgar George	Yokohama.
George Gilpin	Bradford.
H. M. Goodman	Greenwich.
George Gosselin	Cólombo, Ceylon.
John Hall	Newcastle-on-Tyne
H. S. Hassard, Lieut.	66th Regiment.
J. W. Ingram	General Post Office.
John Lavender	Manchester
D. E. McGauran	Melbourne.
P. J. Mosley	Newcastle on-Tyne
John Muirhead, jun.	Westminster
F. Wm. Ogg	Regent Street
Alfred S. Page	Silvertown.
E. W. Parsons	Adelaide Road
John Penman	St. Thomas, West Indies.
W. R. Phillips	Paumbaum, India.
H. R. Rich	Calcutta.
F. L. Robinson	The Lizard.
George Simpson	Manaar, Ceylon.
Robert Stout	Lerwick, Orkneys.
G. E. Stow	West Strand.
Richard Theiler	Canonbury.
C. F. Venndt	Aberdeen.
W. M. Warden	Birmingham.
R. J. Waters, B.A.	Osnaburg Street.
George Watt	Mauritius.
Gordon Wigan	Campden House, Kensington.

The Meeting then adjourned.

The Second Annual and Twentieth Ordinary General Meeting was held on Wednesday evening, the 10th December, 1873, Mr. W. H. Preece, Member of Council, in the Chair.

The Chairman said, this being the annual meeting for the election of President and Council for the ensuing year, the ballot would open forthwith and remain open until half-past eight o'clock, when it would be closed.

Lieut. Ramsey, R.E., and Mr. W. H. Winter having been appointed Scrutineers of the voting papers, the balloting was then proceeded with.

The Chairman read the Annual Report from the Council, as follows:—

Gentlemen,

Agreeably to our Statutes the time has again come when your President and Council must resign their offices and account to you for their stewardship.

From the establishment of the Society the Institution of Civil Engineers have lent us a kind helping hand; the fostering care of the parent Institution may be said to have done more towards the present vitality of the Society than any other external assistance. It has been by their kind permission that we have been able from the first to hold our meetings in this noble theatre, where our members have collected together to hear such papers read as have been brought before them. The use of the theatre and all lighting arrangements have been placed at the disposal of our Society, free of all expense, by the parent Institution; this, it must be remembered, was as great a service as could possibly be wished for by a new Society, and we trust that our meeting will not separate this evening without testifying unanimously their high appreciation of the kindness of the Institution of Civil Engineers.

We have great cause for comfort and congratulation in the continued prosperous increase of the Society, and its rapid and constant growth. Although the increase is not so great as in the preceding year, it is nevertheless highly satisfactory; but it must be remembered that, as year succeeds year, the increase in the number of the Society's members must naturally decrease as we bring those of our profession within the folds of the Society. The total increase over the preceding year of members of all classes has been (including such gentlemen as will be balloted for this

evening) 170; the total number of all classes at the present time being 522. The relative numbers at the annual meeting in 1872, and at the present time, are as follows:—

	1872.	1873.	Increase.
Honorary Members - - - -	0	3	3
Foreign - - - - - -	25	57	32
Ordinary - - - - - -	155	185	30
Associates - - - - - -	170	271	101
Students - - - - - -	2	7	5
	352	523	171

From these figures it will be seen that, as the Society gets better known, so its scope is increased, the addition of Foreign members being highly promising; whilst at home a very large increase in Associates is apparent. We must not, however, forget that, whilst new members are constantly joining us, death has been busier with us than such a young Society can well afford. That well-known name—the father of Electric Telegraphy—Sir Francis Ronalds, has passed from amongst us; his name was associated with Telegraphy from so old a date that he was the one link which bound the present to the past. We miss also M. Charles Lendi, of Berne, one of the original members of the Society, who was well known in connection with the International Bureau; Sir Donald McLeod, Captain Langham Rokeby, Mr. George Saward, connected from its commencement with Atlantic Telegraph enterprise, and to whom telegraphy to America must always owe a large meed of praise for his incessant exertions; and, lastly, Mr. Theiler, who as a practical telegraphist had long resided in this country.

The financial state of the Society is eminently satisfactory. From the present position of affairs it appears that we have a balance in hand of £360, exclusive of the value of the Journal, and of subscriptions over due. At the first ordinary meeting in January a detailed statement of accounts will be laid before the members. The funds of the Society are largely indebted for their present flourishing condition to the liberality of two of our members—Mr. C. F. Tietgen and Mr. H. C. Erichsen—who have presented the Society with £100 each, in addition to their becoming life members.* Such liberality is worthy of imitation by gentlemen

* It will be remembered that Dr. C. W. Siemens, in the previous year, presented the Society with a similar sum.

belonging to the Society who are natives of this country. In addition to the above names, Professor Clerk Maxwell has become a life member.

For some time past the Council, by means of their Sub-Committee, have been endeavouring to obtain rooms suitable for a library and office, which would be used as a council-room, and also a general reading-room for members. They have at length secured commodious and convenient rooms in No. 4, Broad Sanctuary, which are now being prepared ready for being taken possession of by the Society. They will be furnished as early as possible, and it is to be hoped that they will be opened early in the year.

The following additions to the Society's Library have been made:—

PRESENTS.

"Electrical Testing." Captain Hoskiar, from the Author.

"Reports on Electrical Standards." Edited by Professor F. Jenkin, by the Editor.

"Report from the Select Committee on Telegraph Communications," New South Wales Legislative Assembly. By F. R. James.

"Report of the Victorian Post-Office and Telegraph Department."

"Catechism on the Electric Telegraph." By Dr. Zetysche. By the Author.

"Electrical Pamphlets." By Dr. Zetysche.

"The Italian Semaphore Apparatus." By M. d'Amico.

"Cables Telegraphiques." By C[lr]. Foucaut. "Sur la Resistance électrique des Metaux." By M. Benoit. By M. Francisque Michel.

"The Transactions and Proceedings of the London Electrical Society," from 1837 to 1840. By C. V. Walker, F.R.S.

"On differential Galvanometers." In two parts. By the Author, Louis Schwendler.

"Instructions for Testing Telegraph Lines." By the Author, Louis Schwendler.

"Statistics of the Royal Italian Telegraphs for 1871." By M. d'Amico.

"The Hughes Apparatus," illustrated. By M. Miriel.

Now that the Society possesses room, it is to be hoped that members will not forget to aid the Council in increasing the Library.

In our statutes it will be found that we are permitted to elect Honorary Members, who shall be distinguished individuals, eminent for science and experience in pursuits connected with the profession of telegraphy. In

the previous year your Council had this subject under discussion, but postponed the consideration of the question until the numbers and scope of the Society had increased, so that the appointment of such members might be considered as an honour to them as well as to the Society. During the past year the question was again re-opened, and your Council had the satisfaction of proposing the following distinguished names as the *first* Honorary Members of the Society of Telegraph Engineers:

Sir George Biddell Airy, K.C.B., Astronomer Royal.
General Sir Edward Sabine, K.C.B., R.A.
Professor William Weber, F.R.S.

The nominations were duly acknowledged, and the appointments accepted, and the Council have to congratulate the Society upon the fact that the Honorary Members first created are men of such eminence in the world, and who have so greatly distinguished themselves in science and in pursuits which have had so material and so important a bearing upon the present successful state of telegraphy.

In order to cultivate our foreign relations, and to enable us to rise to the dignity of an International Institution, your Council have had under their consideration for some time the idea of a system of Corresponding Secretaries; they have, however, decided upon offering to eminent telegraph officials in Foreign States, and in our own Colonies, an appointment, to be termed "Local Honorary Secretary." For this purpose the following communication was addressed to the several heads of departments:—

SOCIETY OF TELEGRAPH ENGINEERS,
5, Westminster Chambers, S.W., London.

"SIR,

"I am desired by the President and Council of the Society of Telegraph Engineers to invite your co-operation in extending the Society, and in increasing the number of its members, and consequently the scope of the utility of the Society.

"It is proposed to establish in each country an honorary appointment, to be filled by a member of the Society, whose position, telegraphically considered, will enable him to reflect honour upon the Society.

"The appointment will be termed 'Local Honorary Secretary,' and the duties attached to such office are but nominal, the main object being to advance the Society by obtaining an increase in members, and to act, when required, between the Council and the various foreign members resident in the same country as the Local Honorary Secretary.

"I have the honour, therefore, to ask, on the part of our Council, your acceptance of the appointment of the Society's Honorary Secretary for   , and to assure you that your acceptance of it will meet with the greatest gratification, as they believe it will tend materially to the Society's advancement.

"I have the honour to be, Sir,

"Your obedient servant,

"GEO. E. PREECE, Secretary."

Previously, however, the Council had elected the following gentlemen:

Colonel ROBINSON, R.E., Director-General of Indian Telegraphs, *Honorary Secretary for India.*

Mr. W. E. AYRTON, Professor of Natural Philosophy, The College, Yokohama, *Honorary Secretary for Japan.*

Replies to the above circular have been received, and the following subsequent appointments made:

M. LE COMMANDEUR D'AMICO, Director-General of Telegraphs, Italy, *Honorary Secretary for Italy.*

M. NIELSEN, Director-General of Telegraphs, Norway, *Honorary Secretary for Norway.*

M. DÉLARGE, Inspector of Telegraphs, *Honorary Secretary for Belgium.*

The Council have every reason to hope that before long the various appointments for the different countries and states will be filled, and that the Society will benefit in a corresponding manner.

Certain alterations in the rules and regulations were considered advisable, and a sub-committee was appointed for the purpose; they have been subsequently brought before the meeting, after sundry alterations proposed and approved in Council. Of the alterations made, the following are the most important: Rule 2. "Every member shall have been previously elected as an Associate;" these words have been left out so as to leave it within the discretion of the Council to elect a member at once, the double election having frequently proved an objection

The amount of composition for Life Members has been reduced from twenty-five guineas to twenty-one pounds, and foreign members and associates are now allowed to compound for the sum of ten pounds. It is believed that this reduction will benefit the Society materially, so soon as it generally becomes known.

In addition to the above, clauses have been inserted respecting the funds and property of the Society, and the due regulation of the same.

Sundry other minor regulations have been altered, which your Council believe will materially contribute to the interests of the Society.

A *soirée* should have been held this year, but from the unfinished state of the new Post Office buildings, where it was intended to be held, was unavoidably postponed. The constant attention of Mr. Scudamore to his official duties has unfortunately prevented him from attending to the duties of presiding over our meetings as often as he would wish to have done.

The Council have recently received from the Commissioners of the International Exhibition of 1874 the following letter:—

UPPER KENSINGTON GORE, LONDON, S.W.,
27th November, 1873.

"SIR,

"The Committee for Class 14, Scientific Inventions, of the London International Exhibition of 1874, at their recent meeting, recommended Her Majesty's Commissioners to apply to the Society of Telegraph Engineers to exhibit in the class in question such novelties as may have lately come under their notice, or to advise the Committee how to obtain any such novelties for exhibition.

"I am, therefore, to express the hope that your Society will be willing to render Her Majesty's Commissioners the benefit of their valuable assistance in this matter.

"I have the honour to be, Sir,
"Your obedient servant,
"T. A. WRIGHT,
"Secretary for the International Exhibition."

The Secretary of the
Society of Telegraph Engineers,
2, Westminster Chambers, Victoria Street.

The Council appreciate the offer made, and consider it would be most important to the interests of the Society that they should be prominently represented amongst the class of inventors, &c. in the new Exhibition: they therefore consider it desirable that all the members of the Society should be written to, in order that, should they have inventions or apparatus of any novel kind, they would undoubtedly add to the lustre of the Society by exhibiting under their auspices. The Secretary has been directed to correspond with all members on this point.

The following will give the details of the various meetings held during

the past year, and the papers read on the several evenings, arranged chronologically. They are—

PAPERS READ.

JANUARY 8.—Presidential Address.

"On a New Method of Measuring Battery Resistance."—Dr. WERNER SIEMENS.

"On the Tray Battery," and "On a New Form of Joule's Tangent Galvanometer."—Sir WILLIAM THOMSON.

"On Lightning and Lightning Conductors."—JAMES GRAVES.

JANUARY 22.—"On the Measurement of Electro-Static Capacity," and "On Testing Batteries."—Sir WILLIAM THOMSON.

"On a Common Source of Error in Measurement of Currents of Short Duration when using Galvanometers with Shunts," and "On an Instrument for the Measurement of Electro-Potentials."—LATIMER CLARK.

FEBRUARY 12.—"The Effect of Light upon Selenium."—WILLOUGHBY SMITH.

"On the application of Iron to Telegraph Poles."—Major WEBBER, R.E.

"On Telegraph Poles."—Lieut. JEKYLL, R.E.

"On Iron Telegraph Poles."—C. W. SIEMENS, F.R.S.

"On the Riband Telegraph Post."—ROBERT BRISTOW LEE.

FEBRUARY 26.—"On Telegraph Poles."—Adjourned discussion.

MARCH 12.—"Earth Currents in the Orkneys."—ROBERT STOUT.

"Earth Currents, and on their Bearing upon the Measurement of the Resistance of Telegraph Wires in which they exist."—G. K. WINTER.

"On the Earth Currents observed in the Atlantic Cables."—JAMES GRAVES.

MARCH 26.—"On a Method of Testing lengths of highly Insulated Wire."—FLEEMING JENKIN, F.R.S.

"On some points in connection with the Indian Government Telegraphs."—W. E. AYRTON.

APRIL 9.—"On a Bell-Alarm for Submarine Cables."—W. F. KING.

"On the Mechanical Tests of Iron Wire."—R. S. CULLEY.

APRIL 23.—"On the Block System of Working on Railways."—W. H. PREECE.

"On the Block System of Working on Railways."—Captain MALLOCK.

MAY 14.—"On the Block System of Working on Railways."—Adjourned discussion.

NOV. 12.—"On the Quadrant Electrometer."—JOHN MUNRO.

NOV. 26.—"On Underground Telegraphs."—G. E. PREECE.

DEC. 10.—"On Underground Telegraphs."—Adjourned discussion. Annual General Meeting.

Of the Journal of the Society two numbers have appeared already this year, the first, No. 3, completing the Society's first volume; the third number for this year is now in print, and will shortly be published.

The following original communications have appeared, or are in course of publication:—

"Visual Telegraphy."—Capt. COLOMB, R.N.

"A Normal Resistance Thermometer."—ROBERT SABINE.

"Resistance of Wires."—JAMES GRAVES.

"On the Measurement of the Internal Resistance of Batteries."—H. R. KEMPE.

"Kenosha Insulator."—JAMES HASKINS.

"The Action of Oak upon Earth Wires."—JAMES SIVEWRIGHT, M.A.

"Nautical Telegraphs."—Capt. COLOMB, R.A.

"On the Internal Resistance of Batteries."—JAMES GRAVES.

"On Coiling of Submarine Telegraph Cables."—C. L. M.

"Duplex System of Telegraphy on Submarine Cables."—C. V. DE SAUTY.

"On Lightning Protectors."—JOHN FLETCHER.

"Tables to facilitate the Calculations of Strains on suspended Iron or Steel Wires."—ROBERT SABINE.

"On Measuring Differences of Electric Potential."—Professor ADAMS.

In addition to the above the Journal contains a varied amount of "extracts and abstracts," containing matter of general and particular interest to the members of the Society. The Council are anxious to make the Society's Journal a high-class one, and they would wish to impress upon members the desirability of forwarding to the Secretary communications on any new or interesting points which may arise in the course of telegraphic experiences or of electrical investigations.

The Council wish to bring before the members the importance of their contributing papers and communications to be read before the meetings of the Society on subjects connected with the science of electricity and the practical progress of telegraphy. To a young Society the importance to be derived from the assistance of members in this respect cannot be over-rated. And those who attend to hear papers read should bear in mind, that, so far as they are able, they themselves should aid in the welfare of the Society by contributing such facts and experiences as come within their own personal knowledge. Great difficulty has been experienced in this respect. Members have been applied to personally, and the Council now have to appeal to them individually, to contribute for the interests of science generally, and telegraphy in particular, such papers as they can. In the course of next Session they propose issuing invitations for the presentation of papers on specific points. Ultimately it is probable, that, as the finances of the Society improve, premiums will be offered for such contributions as are deserving of special merit. It may, however, be specially mentioned, that such papers read before our meetings as have already appeared in the Journal have called forth special commendation and have been much sought after.

In resigning his functions for the present year, the President has great gratification in announcing that Sir William Thomson has kindly consented, subject to your election, to become the President for the ensuing year, and it is anticipated that the presidency of a naturalist and mathematician, celebrated over the world, will materially conduce to the interests of the Society.

The Council have every reason to hope that the succeeding year will open as brightly as the present did, and that the numbers of the Society will increase as greatly as they fairly anticipate, and that the Society itself will enlarge its scope for utility and the general benefit of science and the universal progress of telegraphy.

STATEMENT OF RECEIPTS

For the

	£	s.	d.	£	s.	d.
To Balance, January 1st, 1873 . . .				41	1	7
" Donations	200	0	0			
" Life Members' Composition . . .	78	15	0			
				278	15	0
" Subscriptions				573	0	1
" Publishing Fund—						
Subscription				70	10	0
Sale of Journals				28	4	6
				£991	11	2

Assets of the Society on the 31st December, 1873.

	£	s.	d.
Cash Balance	361	18	10
Arrears, Subscription	158	15	5
" " Publishing	101	8	6
Value of Stock of Journals 1, 2, 3, 4 . .	228	0	0
	£850	2	9

AND EXPENDITURE
Year 1873.

	£ s. d.	£ s. d.
By Expenditure—		
Salaries		155 0 0
Shorthand Writer (Reports) . .	21 0 0	
Clerks, Messengers, Tea and Coffee at Meetings	20 2 6	
		41 2 6
Postages and Sundries . . .	43 16 8	
Loss on Exchange	2 4 5	
Refunding Composition, &c. . .	17 18 0	
Furniture for Office	11 10 0	
		75 9 1
Stationery	75 15 2	
Publication of Journal . . .	282 5 7	
		358 0 9
		629 12 4
Balance Credit . . .		361 18 10
		£991 11 2

C. E. WEBBER, Major R.E., *Treasurer.*

We have compared the above Account with the Vouchers and the Cash Book, and find it to be correct, leaving a balance in the hands of the Treasurer of three hundred and sixty-one pounds, eighteen shillings, and ten pence.

(Signed) J. WAGSTAFF BLUNDELL, F. C. DANVERS, } *Auditors.*

GEO. E. PREECE, *Secretary.*

The CHAIRMAN said they had one important duty to perform, that was to pass a vote of thanks to the Council of the Institution of Civil Engineers, and he should be glad if some member would do so.

Mr. GRAY said the resolution of thanks referred to by the Chairman had been entrusted to him, and he had great pleasure in proposing it.

It was as follows:—

"That the Society of Telegraph Engineers having now arrived at the third year of its existence, and having during that period been materially aided in its development by the fostering assistance of the Institution of Civil Engineers, tenders its thanks to the parent Society for the encouragement and aid which has thus been liberally given to Telegraphy as a science and branch of engineering.

"Not only would the Society of Telegraph Engineers acknowledge the advantage thus brought within the reach of a large number of Telegraphists and Electricians at home, but it also wishes especially to express its conviction that the generous support of the Institution of Civil Engineers has been the means of increasing the interest of all countries in Telegraphic Progress in the United Kingdom."

There was no doubt that the members would perfectly agree with him that this ought to be a very warm and hearty vote of thanks. The Council of the Institution of Civil Engineers had been exceedingly kind to foster a young Society like this, and give them the use of that magnificent hall without even charging them for gas; and there was not a doubt the meeting in such a splendid room, ably aided by the Council of the Society, had greatly conduced to its prosperity, and they could not do less than pass a warm vote of thanks to the gentlemen to whose kindness they were so much indebted.

Mr. WALTER HANCOCK said he had the greatest pleasure in seconding the resolution. Many members present would recollect that previous to the enlargement of that hall many subjects of interest in relation to Telegraphy were introduced and discussed at the meetings of the Institution of Civil Engineers; and, with the exception of the Society of Arts, no other Society took any active interest in the progress of Telegraphy. It was therefore particularly appropriate that a young Society, not then in possession of funds to provide a room for themselves, should hold its early meetings under the kind auspices of the parent Institution, that being the relation in which they might regard the Institution of Civil Engineers as standing towards this Society, and peculiarly appropriate also that the meetings should have taken place under that noble

roof. He had great pleasure in seconding the resolution proposed by Mr. Gray.

The CHAIRMAN then declared the motion to be carried with acclamation.

The discussion on Mr. G. E. Preece's paper on Underground Telegraphs was resumed.

Mr. ANDREW BELL (responding to the Chairman's invitation to communicate the result of his experience with underground wires in works with which he was connected in 1862) said at the time the Chairman referred to, underground telegraphy was in its infancy. Scarcely anything was then known about it, and a few years after the wires were laid they failed. Since then he had been very little engaged in underground works. He laid down some wires on the London and North Western Railway at Liverpool about the time referred to by the Chairman, but it was only a small affair.

Major WEBBER, R.E.—There is one point in Mr. George Preece's paper which appears to me to require explanation. I may have missed in the reading of the paper the explanation which I require, and I have not been able to find that Mr. Preece has done what seems to me to be essential in conducting a satisfactory test of the cable, which he superintended the laying down of, between Manchester and Liverpool. He describes to us with great care the mode in which he tested the joints of each section, but he does not describe to us the way in which he tested the joints between section and section, and I do not see that the mode in which he tested the joints of each section is applicable to the testing of the joints between section and section. I may be wrong, but it seems to me to require some additional explanation, and that the cable *was* imperfect. I do not mean it *is* imperfect, but, for want of a test of these joints being completed, the joints of the cable were not really tested from end to end. No doubt Mr. Preece will be able to explain what I have been unable to understand in his paper, or that this omission has been made up in some other way. I think the Society has to congratulate itself upon eliciting such an interesting practical paper as this is. I have had very little experience myself in the early stages of underground telegraphs, but I should like to be one of those who bear witness to the great perfection it has reached in late years. I do not myself like the employment of an invisible conductor, except under the sea; but unfor-

tunately for us in this country it is necessary to employ underground telegraphs, and no doubt a great many here to-night are perfectly well aware that telegraph companies obtained legal powers by which they could lay down underground telegraphs almost where they liked, but that the employment of overhead telegraph lines could only be adopted with the sanction of the inhabitants of the locality in which it was proposed to erect the line of telegraph. Therefore it was found that underground telegraphs, meeting with less opposition in the extension of telegraphs with regard to large lines through the country, they were often adopted, while the more expensive means would have been avoided. To this I think we may ascribe the development of underground telegraphs in this country. No doubt if you travel from one end of the continent of Europe to the other you will not find underground telegraphs used to anything like the extent they are in this country; whereas in our own case we are obliged to lay our telegraphs underground in towns where there was nothing else to compel us to do so except the supposed eyesore to the inhabitants of the locality of overhead lines; but on the continent of Europe, the telegraphs being in the hands of a more arbitrary Government, there is no compulsion to use underground telegraphs; and, moreover, they do not use underground lines in places where we consider them necessary, but in the towns overhead lines are carried out, and that not only in towns where appearances might not be a matter of much consideration, but also at the beautiful bathing and other places of resort, where in this country the suggestion of overhead telegraphs would be considered an act of barbarism. Therefore I think the development of underground telegraphs being in this anomalous state in this country the immediate question that arises is, How are these underground telegraphs to be maintained in efficiency? Now we have the data under which we can maintain a line of overhead telegraph in efficiency for a series of years. We know, as the materials of which they are constructed begin to deteriorate, we can replace the material without interfering with the working of the line. We know that in the process of maintenance the localising of faults is more simple in an overhead line than in an underground line; in fact, we have all the data for maintaining efficiently an overhead line, whereas we have not all the data for doing the same work in an underground line; for this reason, the maintenance of the old underground lines was notorious for its inefficiency. A few years back they were all but useless, and now-a-days underground lines break down from the fact that the maintenance cannot be current, or on a system to

counteract gradual and certain deterioration. Hence we may recognise the great importance of making thoroughly good joints. We know the necessity for this was recognised many years ago; at the same time we know that in many of the lines in towns in this country the joints which were made at various places at short intervals were found to be of such a nature, that insulation was almost impossible to be expected. Therefore I think what should follow Mr. Preece's most interesting paper—in which he has described the construction of an underground line in the most perfect way,—what should follow that, would be some suggestions in connection with the maintenance of those lines; for we know an immense amount of money has been spent on lines of this description, and that their life has been rarely more, if so much, as that of overhead lines. It is a great pity that lines of such an expensive nature should be put down, and so little as yet suggested as to the best mode of maintaining those lines. In other words, supposing an overhead line maintained in an efficient state only requires to be renewed in ten years, we want to know how long such a line as that described by Mr. Preece will last. We ought to be able to form an idea of the maintenance, so that renewal may be put off as long as possible, and that the expense may not be greater than the necessity of the case requires.

Mr. ROBERT GRAY: The manner in which Mr. Preece has described joint-testing in sections, in which the two ends are not to be got at, perhaps would be best managed as it is done at sea. That is, they make the joints on board ship, not having the two ends on board, by taking a per-centage test after two or three minutes in the trough; and then, by putting the two ends of the line to earth, taking the per-centage again. This might, to some extent, meet Major Webber's views.

Mr. WALTER HANCOCK. I must say I have been exceedingly pleased with the paper which Mr. Preece gave us this day-fortnight, and with the immense care and skill taken, and the scientific appliances that were devoted to the manufacture and the laying of this particular line of telegraph. I am sorry to say I have not had time to look through this paper since it was sent to me; but I may make a few remarks following those which were made at the last meeting upon the early stages of underground lines in Germany, with respect to the early stages of the manufacture in England. Mr. Preece has stated that the various underground telegraphs may be said to date from 1853. I will endeavour to give you a little information with respect to what was done prior to 1853. Shortly after Professor Faraday told us that gutta percha was a most useful and avail-

able dielectric, the Electric Telegraph Company turned their attention to the use of it for line-insulation. Owing to the objections that were made in a city like London to overhead telegraphs it became very desirable that subterranean lines should be used. Mr. Hatcher, who was in the employ of the Electric Telegraph Company, applied to my father to know whether it was possible to turn the gutta percha, on which he was then working, and had spent some years, to account for the purpose of insulating wires. That was about the end of 1846, or the commencement of 1847. At that time one of the processes for making a round gutta percha core was to run it through two semicircular rollers. The earliest specimens of covered wire made in 1847 were wires rolled simultaneously with soft gutta percha through the middle of rollers. One great objection to wires covered in that way was that at the junction of the rollers the gutta percha opened and exposed the copper. Many other experiments were tried. At length, in the month of July, 1848, my father invented the machine which has ever since been used for the manufacture of all subterranean gutta percha covered wires; and after the patent was taken out experiments were made to cover several wires in one core of gutta percha. I had intended to bring with me a specimen of that work, but I have not been able to obtain it. The cable consisted of seven wires inclosed in a core about 5-16ths of an inch diameter. In July, 1849, Mr. Reid, whose name will be known to many present, and who was under a contract to lay down some lines for the Electric Telegraph Company, applied to my father to know if he could manufacture the wires for him. You will, no doubt, recollect that the earliest wires laid under the streets of London were three copper wires laid in one core, and I have brought with me a sample of that. The wires used were No. 16 gauge, and with a covering of gutta percha; the core was half an inch diameter. This was found to be a useful covering, but some long time after it was customary to run the wire through the machine and lay on one coat only. We never professed to put two or more coats on. With regard to these wires there were two or three objections, electrical and mechanical, more especially the latter. Another great defect was that so much material was thrown away. The gutta percha at that time cost only 2*s.* 3*d.* per lb. instead of 7*s.* or 8*s.* It became necessary for the Company to see how they could get their lines manufactured at smaller cost, and for this purpose two schemes were proposed. The first was to substitute iron wire for copper wire. Afterwards it was proposed to put four wires in one

core, the weight of the cable so made being about 415 lbs. per mile; but the mechanical objections to that were much increased. Shortly after that they found, in consequence of the expense of repairing this line, throwing three wires out of place to repair one, it was so inconvenient and costly that single wires began to be used, and about the year 1861 single wires with two or more coatings of gutta percha were first used. Mention has been made as to the early wires used on the continent. The earliest specimens which I have seen were generally indifferently coated with a single covering of gutta percha. Whether it was from motives of economy, or supposed superior electrical advantages, the samples I saw were made of a compound of gutta percha and sulphur, not vulcanised, but a mechanical mixture of sulphur with gutta percha. That would no doubt give a good dielectric, but in a short time the sulphur made the gutta percha very brittle, and the sulphur coming in contact with the copper caused a sulphuret of copper to form which ate away the wire, and many of the failures on the continent, I believe, arose from the attempt to use sulphur with gutta-percha. It was found that some of the wires laid prior to 1853 failed from having been laid in leaden troughs. This was especially the case with lines laid on the London and Brighton, and the South Eastern Railways; these were laid in leaden troughs, and it was found that the lead in time dissolved the gutta percha, and the wires in many places ran together. One of the difficulties which Mr. Preece referred to in the maintenance of gutta percha underground wires caused a great deal of trouble to the late Mr. Highton, who discovered that on underground wires, unprotected, which ran near an oak-tree, a fungus formed upon the gutta percha which caused rapid decay, whilst in other places the wires were perfect. I may mention, with regard to the first submarine cable laid in the English Channel in 1850, that it consisted of a single No. 16 wire, coated with a covering of gutta percha half an inch diameter. That cable, as we know, had a short existence. This was occasioned by the condition in the concession that a message was to be conveyed across the Channel within a year of the concession being granted. There was difficulty in raising the money for making the cable. It was thought to be an extraordinary risk; but a cable was made consisting simply of a copper wire, covered with a single coating of gutta percha; but it carried the message, and saved the concession for ten years.

Mr. Hancock wished to mention that one of the earliest proposals for underground telegraphs was that of Mr. Francis Whisham, formerly

Secretary to the Society of Arts, and which was seriously entertained at the time, although telegraph engineers of the present day would hardly be inclined to look upon it with favour. That plan was to make a solid core of gutta percha, with four open grooves in it, to lay the copper wires in each of these grooves, and then fill up the upper part of the grooves.

The CHAIRMAN stated, that, as several members who were anxious to discuss this subject were deterred from being present by the unfavourable weather, it was proposed to adjourn the further discussion till the next meeting.

The CHAIRMAN then read the Report of the Scrutineers as follows :—

President.

SIR WILLIAM THOMSON, F.R.S., LL.D.

Past-Presidents.

FRANK IVES SCUDAMORE, C.B.
CHARLES WILLIAM SIEMENS, F.R.S., D.C.L. M.INST.C.E.

Vice-Presidents.

THE LORD LINDSAY.
LATIMER CLARK, M.INST.C.E.
R. S. CULLEY, M.INST.C.E.
PROFESSOR G. C. FOSTER, F.R.S.

Members of Council.

PROFESSOR ABEL, F.R.S.
MAJOR MALCOLM, R.E.
W. H. PREECE, M.Inst.C.E.
ROBERT SABINE, C.E.
CARL SIEMENS, M.Inst.C.E.
C. E. SPAGNOLETTI.
COLONEL STOTHERD, R.E.
C. V. WALKER, F.R.S.
MAJOR C. E. WEBBER, R.E.
WILDMAN WHITEHOUSE.
PROFESSOR WILLIAMSON, F.R.S.
CROMWELL VARLEY, F.R.S., M.Inst.C.E.

Associates.

ANDREW BELL.
DR. A. MUIRHEAD.
LIEUT. WATSON, R.E.

OFFICERS.

Auditors.

J. WAGSTAFF BLUNDELL.
FREDERICK C. DANVERS (India Office).

Hon. Treasurer.

MAJOR C. E. WEBBER, R.E.

Hon. Secretary.

MAJOR FRANK BOLTON.

Secretary.

GEO. E. PREECE.

On the motion of Major Webber a vote of thanks was passed to the Scrutineers for the able manner in which they had discharged their duties.

The following Candidates were balloted for and declared duly elected :—

As Foreign Members :—

F. C. Guilleaume	Cologne.
Gustave Hoffmier	Shanghai.
M. G. Kellogg	Chicago.

As Members :—

W. W. Cargill, F.R.G.S.	Yokohama.
E. C. Cracknell	Sydney.
W. F. Cracknell	Brisbane.
Charles A. Gerhardi	Dublin.
S. McGowan	Melbourne.
Charles Todd, C.M.G.	Adelaide.

As Associates :—

James Allen	Buenos Ayres.
W. H. Bailey	Manchester.
James Brogden	Westminster.
G. G. Charles	India Government Telegraphs.
W. C. Collins	Old Jewry.
Henry Davy	New Charlton.
John Douglas	Calcutta.
A. Eden	Edinburgh.
Robert Gray	Blackheath.
A. Hilliard	Silvertown.
E. T. Kaiser	New Charlton.
B. C. Molloy	The Temple.
L. Schaefer	Tokei, Japan.
W. Slater	Rio Janeiro.
J. T. Sprague	Birmingham.
J. O. Spratt	Penzance.
D. Stewart	Glasgow.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

EARTH CURRENTS.

The following observations of "earth currents" may prove interesting, and as a proof of the remarks I made relative to earth currents during snow-storms:—

1873. March 9th, 8·30 p.m.—Magnificent aurora, peculiar form, brilliant green arches in northern part of sky, and pale green haze in southern half, needle of instrument on Scalloway Line lying across dial to left, needle of instrument on Unst Line deflected about 45° left, and reversing quickly left and right, *i.e.* + and — . —

10·30 p.m.—Morse instrument on Main Line affected, armature working, needle of galvanometer vibrating like a pendulum, *i.e.*, current reversing quickly, needles on Scalloway and Unst Lines reversing so quickly, that, but for difference of action of current from battery current, would almost think some station was working: Sunday night, so cannot get any of the stations to ascertain how their needles are affected.

13th March, 2·20 p.m.—Heavy snow-shower, needle of instrument on Scalloway Line deflected fair across dial to left, current went off as shower ceased, needle of instrument on Unst Line not affected.

3·30 p.m.—Another snow-shower, wet flakes, slight current on needles of both instruments, deflection about 10° left (+ current). When shower nearly over, needle of instrument on Scalloway Line returned to zero, and that on Unst Line reversed about 5° right.

24th March, 10·40 a.m.—Strong earth current on Scalloway Line, needle deflected about 45° left (+ current), but clerk at Scalloway says no current observable there, or, if any, so slight as to be scarcely observable; he thought that needle was deflected about an eighth of an inch to the left, *i.e.*, nearly 2°. Between this and April 18th I have several notes of earth currents, but none very marked; currents generally steady, or, when reversing, doing so in from 10 to 20 minutes.

18th April, 10·20 p.m.—Brilliant aurora, flashing rapidly from S. to S.E., very little in north, only a very thin luminous haze, seemingly distant. Earth current on all lines, needle of instrument on Scalloway

Line deflected about 70° right, that on Unst Line about 30° right; very little on galvanometer of Morse; current steady.

10·27 p.m.—Needle of instrument of Scalloway Line vibrating over arc from 40° to 50° right, as if current was increasing and decreasing rapidly in electromotive force, vibrations not taking longer than three seconds.

10·30 p.m.—Current again steady.

23rd April, 4·45 p.m.—Snow-shower on, small flakes, needles of instruments on both Scalloway and Unst Lines deflected about 30° right (—current).

5 p.m.—Current reversed, deflection about 10° left, shower about done.

6·40 p.m.—Snow-shower on, current on both lines, needles deflected to right (—current). Needle on Scalloway Line deflected about 30°, that on Unst Line about 40°.

6.45 p.m.—Shower ceased somewhat, deflection of needles less, snow varying in density and also in character of flakes, being sometimes sleety. Current seems to vary in intensity or electromotive force, needles falling and rising gradually.

6·50 p.m.—Current reversed on Scalloway Line, needle deflected about 15° left, but on Unst Line needle still deflected to right about 10°.

6·55 p.m.—Shower ceased; lines almost clear of current.

These observations, I think, tend to confirm my previous ones. With regard to rapid variation of sign of currents, that of 9th March is most marked, and there was no mistaking the steady fall and rise of needles for the deflection produced by battery current. As to the snow, it was melting away as fast as it came to the ground, so that there was no chance of its forming any connection between the wire and the earth.

ROBERT STOUT,
Postmaster.

Lerwick.

EARTH CURRENTS AND EARTHQUAKES.

Valentia.—Diary, November 26th, 1873.—Wednesday.

1·15 a.m.—London complaining of deflections, and requests steady sending.

1·28. a.m.—Have been 17 minutes over one message through deflections.

6 a.m.—Have had to work very steady all morning.

In the *Times* of Friday, November 28, 1873, I find the following:—

"Earthquake.—Mr. Arthur Warden writes to us from Pau-Basses, Pyrenees, that an earthquake occurred at that place on Wednesday. Between 4 and 5 a.m. he was aroused by a violent shaking of the house, which lasted about 30 seconds, the vibration continuing for some seconds longer. The movement appeared to be from east to west. The weather for two days previous to the shock had been warm, even for the South of France, at this advanced period of the year, and the invalids who have sought this favourite resort during the winter months complained of the heat."

JAMES GRAVES.

Valentia, 1*st December*, 1873.

ON A NEW FORM OF TESTING BATTERY.

This battery was designed to obviate the disadvantages met with in the different forms of Daniell's Battery, employed for testing purposes, and more especially on board ship.

Apart from the advantage of its portability, it may be safely said to possess electrical pre-eminence over any modification with which I am acquainted. After being properly set up, it is, both as regards labour and consumption of material, strikingly economical.

A solution of sulphate of zinc, saturated at the boiling point, is diluted with an equal volume of water, and some clean-sifted (pine) sawdust is boiled in this liquid for two or three hours. The sawdust, when required for use, is gently pressed, so as to remove any great excess of liquid.

Another quantity of sawdust, similarly sifted, is allowed to soak for

four or five hours in a warm solution of sulphate of copper, formed by dissolving four pounds of the salt in one gallon of water.

Directions for fitting up the Cells.—The copper plate may be either in the form of a circular disc, or a narrow ribbon bent into a loose spiral. I have not yet been able to ascertain which is the better of the two, but the disc is more easily and uniformly packed. If a disc be used, the bottom of the battery-jar should be evenly covered with sand, so as to let the disc rest perfectly flat, without allowing any chance of air being inclosed by its under surface, which, without the sand, is unavoidably the case from the convexity of the inner surface at the bottom of the jars.

The copper disc or ribbon should be well cleaned with fine emery paper, and not touched with the fingers or anything likely to soil the surface whilst putting the battery up.

Over the copper disc or ribbon is placed a quantity of the sawdust prepared with the sulphate of copper solution, which is firmly rammed down until a layer in depth of about one inch rests on the plate. The solution of sulphate of copper which is squeezed out is poured off. Immediately over this is tightly packed the sawdust prepared with the sulphate of zinc, and when reaching sufficiently near the top of the jar the zinc casting is pressed down and more sawdust packed carefully around it. The liquid squeezed out should not be poured off until the zinc is in its place.

Between the upper part of the zinc and the jar some jute or tow is packed, and also between the tube and the zinc, which when done is closed by pouring some melted paraffin over the zinc and hemp packing, which effectually prevents evaporation.

The zinc should be cast of circular form, slightly conical towards the bottom, with a hole in the centre about three-quarters of an inch in diameter, and a semicircular notch extending the entire depth of the casting, so as to allow the insulated terminal wire from the copper to be brought up. The zincs are well amalgamated and carefully washed. The terminal wire of the zinc should be inserted whilst the casting is being moulded with a boss of the same metal, so as to hold the terminal wire more securely. A coarse thread should be cut on the wire.

I find that the surface of the zincs become more evenly acted upon, when the battery is in action, by being tooled or polished in a lathe. The surface probably becomes more homogeneous by this process, the crystallization which sets in during the slow cooling of the casting being destroyed to a slight depth.

I believe this might be found more advantageous than amalgamating, as I have noticed, after the battery has been in use for some time, that the amalgamated surface excoriates, when the zinc shortly afterwards presents a slightly rough or uneven surface from the indentation.

The hole in the centre of the zinc allows a glass tube, open at both ends, or the stem of a small flask, to be inserted, which should rest on the copper disc.

When a flask is used its neck is loosely plugged with moistened tow, which serves by capillary attraction to draw a supply of sulphate of copper solution from the flask. The flow of liquid by this means will be sufficient, if the flask be taken out and inverted every morning, to allow a supply of air to enter, which will replace the bulk of liquid drawn off.

A more convenient method is to drill a small hole about one-eighth of an inch in diameter in the bottom of the flask, and to paste a piece of paper over it. By making a hole with a fine needle the liquid slowly descends, and may be stopped flowing by closing the hole with a little melted wax or paraffin. A neater method would be the plan of stoppering adopted in certain alkali-metrical apparatus.

The glass tube is, however, the more convenient, as the supply of copper in crystals can be made, or water or solution of sulphate can be poured down. The best plan is to plug the end of the tube with some loose tow, and to drop down a few small pieces of sulphate of copper, according to the work which the battery is made to perform.

As the liquid tends to leave the upper portion of the sawdust, when the flasks are used, thus leaving the sawdust comparatively dry, I prefer using the tube, and keeping sufficient liquid in the cell, by adding water only, so as to be just above the under-side of the zinc.

For use on board ship the cells are arranged in trays, each cell fitting easily into a receptacle of solid paraffin, so that in the event of a cell being broken the testing can still be carried on, or a new cell made to replace it without any great delay.

If, in supplying water to the cells, a little should be spilled between two or more cells, it can be taken up immediately with a little blotting-paper, thus securing the perfect insulation of each cell from one another.

The receptacles are thus formed: A quantity of melted paraffin is first poured into the trays, so as to cover the bottom to a depth of about half an inch; this is allowed to set perfectly hard, when a number of open tin moulds according to the cells to be fitted up are arranged and gently

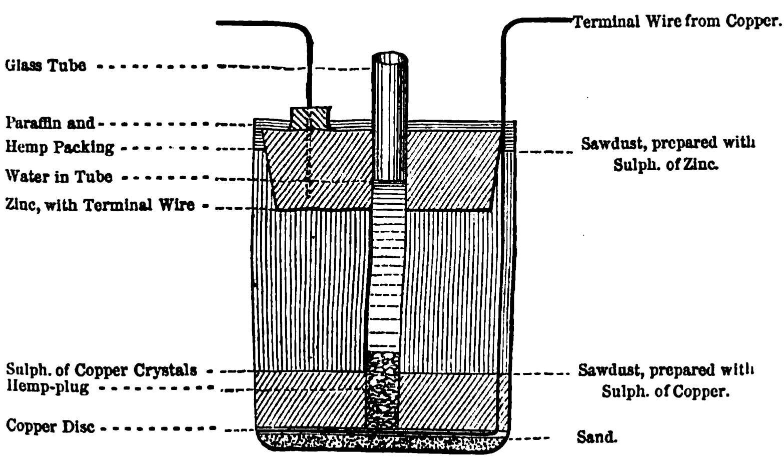
weighted, so as to prevent their shifting when the melted paraffin is poured into the spaces formed between them; when cool the moulds are withdrawn by pouring warm water into them, which softens the paraffin around them sufficiently to free the moulds. The moulds should be a trifle larger than the battery jars to be used.

The electromotive force of this battery is about 1·07 volt per cell as measured by comparison with Mr. Latimer Clark's standard element, and the mean of two hundred cells after being in use for fourteen months at sea was about 1·04 volt. The mean internal resistance of a recently made cell before the tube was adopted in place of the flask was about 22 ohms., and had risen to 44 ohms. in about six months, whereas a similar cell in which the tube was afterwards used fell to about 8 units, which is the general resistance of recently set up cells, with the tube instead of the flask.

It will tend much to the preservation of the battery, where a great number of cells are to be used in series, to disconnect them into tens or twenties when not required.

With a little care in fitting up this battery it is possible to fit up one hundred cells or more at the same time, with no greater variation than ·6 per cent between the cells.

The arrangement of the cell will be more easily understood from the subjoined diagram:—



The economical consumption of the sulphate of copper is not only of pecuniary importance; for it is well known that in any battery where its

addition is in excess of what is actually required we entail not only a waste, but we help to bring about the very opposite of what we want in a battery for testing purposes. After eighteen months' experience on fifteen hundred cells I have not seen a single cell in which a deposit of copper on the zinc has taken place.

THOS. T. P. BRUCE WARREN.

Tamworth House, Mitcham Common.

ABSTRACTS AND EXTRACTS.

SYNOPSIS OF PAPERS ON ELECTRICAL AND MAGNETIC SUBJECTS PRESENTED DURING THE YEAR TO THE PARIS ACADEMY OF SCIENCE.

(*Abstracted from "Comptes Rendus,"* Tome LXXVIII. No. 1.)

ON THE CONDUCTIBILITY OF MAGNETIC TENSIONS.

By M. J. Jamin.

The phrase *coercitive force* is employed to express the difference between the properties of iron and of steel. This force is defined to be the difficulty experienced in magnetising the metal, and the resistance it opposes to the causes of demagnetisation. But the definition is vague and rests upon no definite experiment. To turn to facts. I take a horizontal bobbin, composed of 400 turns of copper wire 2 mm. in diameter; it has a length of 15 c.m. I pass through this coil the current from 20 Bunsen elements, placing in the interior of the coil a prismatic core of soft iron, of the same length as the coil, which becomes an energetic electro-magnet, and takes, for example, a south polar magnetism at its anterior extremity. I approach gradually horizontal bars of iron, of 20 m.m. face; these are subject to magnetic induction, according to known conditions, taking a contrary or northern polar tension at the neighbouring portion, a tension of the same sign (southern) at the distant end. There is a mean line always situate between the centre and core of soft iron; it approaches the core in the same time as the bar itself, and finally disappears when the bar is put in contact with the electro-magnet in such manner as to prolong its surface. At this moment the northern polarity is concentrated entirely at the contact-face, masked by an equal magnetism accumulated on the core. There then remains only a southern polarity prolonged from the core to all points of the bar. Now it is necessary to remark two essentials: (1) the tension, measured by the force of rupture at the contact surfaces, is always the same for the two sides of the contact face, on the part of the core, and on the part of the bar. There is thus a magnetic equilibrium without difference or "fall" between the two metals. (2) The southern polar tension continues the length of the bar as far as its free extremity, nearly without diminution of intensity if it does not exceed 85 centimetres.

We see by this that soft iron possesses the double property of putting itself into equilibrium with a magnet it touches, and of propagating its tension through its substance to great distances; it is this essential character which is expressed by the phrase "*a conductor of magnetic tensions.*"

The distribution of these tensions varies with the form and extent of the bars. It follows a very simple law when they are infinite in length and when they are surrounded at one end by a bobbin such as that described. It is natural to suppose that, in this case, the tension of each infinitely thin stratum is a constant fraction of the preceding stratum, leading immediately to the formula—

$$I = \frac{M}{a^x}.$$

Experiments intended to verify this formula have been made with three bars of equally soft iron of two metres length. The tensions have been measured decimetre by decimetre, starting from the face of the bobbin. The ratios equal to a are given in the following table; they are constant.

IRON (INFINITE BARS).

Distances in Decimetres.	Bar of 20 M.M.		Bar of 15 M.M.		Bar of 10 M.M.	
	Tension.	a.	Tension.	a.	Tension.	a.
0	143·4	1·50	110·2	1·50	70·0	1·57
0·5	113·5	1·39	89·5	1·46	54·5	1·42
1·0	94·6	1·35	73·4	1·38	44·3	1·31
1·5	81·7	1·33	61·2	1·33	38·2	1·33
2·0	70·2	1·31	—	—	33·7	1·29
2·5	61·3	1·29	45·7	1·40	29·0	1·34
3·0	53·3	1·31	—	—	26·2	1·31
3·5	47·6	1·31	32·6	1·38	21·5	1·20
4·0	40·8	1·25				
4·5	36·2	1·26	23·7	1·35	18·0	1·28
5·0	32·7	1·26				
5·5	29·8	1·25	17·5	1·34	14·0	1·40
6·5	23·8	1·31	13·0	1·30	10·0	
7·5	18·2	1·18	10·0	1·25		
8·5	15·8	1·37	8·0	1·50		
9·5	12·3	1·12	5·3	1·26		
10·5	10·5	1·21	4·2			
11·5	8·7	1·34				
12·5	6·5	1·41				
13·5	4·6	1·31				
14·5	3·2	1·37				
15.5	2·3	1·28				
16·5	1·8					

The ratio a is always a little greater in the points neighbouring to the bobbin than at others. I attribute this perturbation, not to the inexactitude of the law, but to the action of the bobbin directly on the parts of the bar adjacent to it.

The ratio is invariable and equal to 1·312 for the three bars of iron studied. That is to say, a metal transmits from one stratum to that sequent to it a fraction of its magnetism that depends upon its nature, but not at all upon its section. This can be submitted to proof; for, if the section be double, it can resolve itself into two halves acting each as a simple surface, and the relation in question does not vary.

But, if a is independent of the section, it is not the case for M. For $x = o$ the following table gives :—

Size of bar.	M.	Ratio of size.	Ratio of M.
20	143·4	2·00	2·05
15	110·2	1·50	1·50
10	70·0	1·00	1·00

For a given intensity of current, M is then proportional to the side b of the bar; it will be equal to 7, if this side be equal to 1 m.m. We have generally—

$$I = 7 \frac{b}{(1{\cdot}312)^x}$$

This formula applies to all bars of iron; it represents for all an unique curve; only the ordinate at the origin varies with the side b and is proportional to it.

Let us now examine steel. I have studied three kinds: the first, but little carburetted, from Niederbronn, had not been tempered; the second, of the same

composition, had been tempered at a red-heat without becoming very hard; the third was a fine specimen of steel.

When we approach one of these bars to an electro-magnet it is subject to the inductive influence, as is iron, with this difference, that the action is less rapid, and that at the contact there is always a more feeble tension than that of the core. There is a difference, a "fall," greater as the steel is harder and richer.

If we study the curves of the tensions at several points of these bars, plunged one end into a bobbin, they satisfy the same formula $I = \frac{M}{a^x}$; but (1) M is as much less, (2) a is as much greater, as the steel is harder and better tempered.

Distances in Decimetres.	Niederbronn non-tempered.		Neiderbronn tempered.		Distances in Decimetres.	Specimen of Fine Steel.	
	Tension.	*a.*	Tension.	*a.*		Tension.	*a.*
0·	109·3	1·56	240·6	2·61	0·0	93·5	2·21
0·5	90·2	1·68	143·7	2·28	0·2	42·3	1·95
1·0	70·0	1·66	92·0	2·08	0·4	21·7	1·94
1·5	53·7	1·71	63·0	2·02	0·6	11·2	1·77
2·0	42·2	1·71	44·2	2·08	0·8	6·3	1·85
2·5	31·3	1·60	31·2	1·93	1·0	3·4	2·26
3·0	24·6	1·73	21·3	2·24	1·2	1·5	
3·5	19·5	1·65	16·2	3·11			
4·0	14·2	1·47	9·5	2·71			
4·5	11·8	1·90	5·2				
5·0	9·7	1·86	3·5				
5·5	6·2	1·44					
6·0	5·2						
6·5	4·3						

These are the properties the phrase *coercitive force* is said to explain. It appears to me more simple and easier to say that soft iron is a good conductor of magnetic tensions, and steel as much less a conductor proportionally as it is harder and richer. I propose then to renounce the words coercitive force, and to replace them by the idea of conductivity, which more or less constitutes the essential property of iron and of steel, and explains all their effects. It is because it is a conductor that iron takes and loses its magnetism as soon as the external cause intervenes or ceases; it is for the contrary reason that steel retains separate the contrary tensions at the extremity of a bar with a power proportional to its length.

("*Comptes Rendus*," Tome LXXVIII. No. 2.)

WATER ELECTRODES AND ELECTRODES OF OTHER LIQUIDS; THEIR PROPERTIES.

By M. Becquerel.

. . . . . . "A test-tube is taken containing an acid solution, in which is plunged a cracked tube containing an alkaline solution; in each of these solutions is introduced a cracked tube filled with distilled water, and in each of the latter is plunged a plate of gold or of platinum, perfectly depolarised (which is easily ascertained by previously plunging the plates into distilled water and introducing them into the circuit of a galvanometer)." . . . . . .

TABLE I.—ELECTROMOTIVE FORCES.

				Mean.	Diff.
1st Couple	Nitrate potash Water . .	− +	47 – 48 – 53 – 48 – 48 – 48	48	18
2nd Couple	Chloride potass Water . .	− +	30 - 27 – 35 – 28	30	
3rd Couple	Chloride potass. Water . . Nitrate potash	− + +	18 – 19 – 19 – 17 – 22	19	
4th Couple	Chloride potass. Nitrate potash	+ −	17 – 20 – 14 – 19	18	

The electromotive forces produced by these four couples prove hydratations, since there can be no other chemical action than that resulting from the reaction of the water upon each of the two salts. As to the electromotive force of the fourth couple it is composed of those that accrue during the mixture of the two salts. These four series of experiments show that the last has for its electromotive force the difference of the two electromotive forces produced by the couples 1 and 2 acting in contrary direction, a result that is still further confirmed by the electromotive force of the third couple, where water is found between the two solutions; the first two give 18, the third 19, and the fourth 18, values sensibly equal. If we work with the fourth couple and two electrodes of water, the effects produced being in contrary direction of the sum of the two first, the result is nil, as would be imagined.

We can then disembarrass the experiment of the electromotive forces resulting from hydratation by employing electrodes of water. For example, let us take a solution of sulphuric acid and a solution of potash. According to the principle previously developed, before the combination and during the excessively short time in which it is effected, the acid forms a new hydrate with the water of the alkaline solution and reciprocally. It is by this means that the combination of the acid with the alkali is effected and produces two electric currents, one proving hydratation and the other the combination of the acid with the alkali.

The first application of these water-electrodes was to ascertain, by means of a cadmium-couple, what were the electromotive forces of a couple (KO 6HO) (SO^3 HO) of which is successively increased, the equivalency of water and of acid as far as SO^3 96HO. In all the experiments the electromotive forces are related to that of the cadmium-couple (cadmium, sulphate of cadmium, amalgamated zinc, sulphate of zinc) taken as 100.

TABLE II.—WATER ELECTRODES.

		Mean.	Diff.
KO 6HO − SO^3 HO +	31 - 31 - 31	31	
			+ 9
KO 6HO − SO^3 2HO +	37 - 39 - 40 - 40 . .	40	
			+ 20
KO 6HO − SO^3 6HO +	62 - 59 - 62 - 57 - 60 - 55 .	60	
			+ 7
KO 6HO − SO^3 12HO +	53 - 53 - 53 - 53 . .	53	
			+ 3
KO 6HO − SO^3 24HO +	50 - 50 - 50 . . .	50	
			+ 4
KO 6HO − SO^3 48HO +	46	46	
			+
KO 6HO − SO^3 96HO +	41	41	

The mean electromotive forces found in the third column are composed of the four electromotive forces proving four hydratations, which annul themselves, being in inverse directions, and of the electromotive force due to the reaction of the acid on the potash, the only one the experiment gives. The means show that in each couple, the electromotive force is in relation with the quantity of water contained in the acid solution. We see, indeed, that from SO^3 HO to 6HO, and probably below HO, the electromotive force increases; that from SO^3 6HO to SO^3 96 HO this force diminishes; and that from 24HO to 48HO the difference is only 4 for 24 equivalents of water, and of 5 for 48 to 96; the differences become less and less.

. . . . From the preceding it is evident that the combination of sulphuric acid with the potash is effected by the intervention of water in producing two hydrates, giving place to two currents; but if the circuit is formed with two plates of gold, instead of water-electrodes, the following results are obtained :—

TABLE III.

Couples.		Electromotive Forces.	Means.	Diff.
1st Couple	$\left\{\begin{matrix}\text{KO 6HO}-\\ \text{SO}^3\ \text{6HO}+\end{matrix}\right\}$	172 - 174 - 176	174	
2nd Couple	$\left\{\begin{matrix}\text{KO 6HO}-\\ \text{Water}\quad+\end{matrix}\right\}$	75 - 76	75·50	106·50.
3rd Couple	$\left\{\begin{matrix}\text{SO}^3\ \text{6HO}+\\ \text{Water}\quad-\end{matrix}\right\}$	31 - 31	31	

The figures 174 represent very nearly the electromotive force of the couple $\left\{\begin{matrix}\text{KO 6HO}\\ \text{SO}^3\ \text{6HO}\end{matrix}\right\}$, which is the sum of the electromotive forces of the two latter couples and of the force resulting from the combination of the acid with the alkali; if then 106·5 is substracted we shall have 67·50, which represents the electromotive force of the first couple disengaged from the electric effects arising from hydration. 67·50 is thus the electromotive force that represents, with a certain approximation, the affinity of sulphuric acid for potash in the conditions of the experiment.

These results show that the method of employing electrodes of water or of other liquids is likely to eliminate errors consequent upon the use of electrodes of gold or of platinum which are sometimes attacked by the solutions.

ON THE DISTRIBUTION OF MAGNETISM IN SOFT IRON.

By M. J. JAMIN.

M. Jamin's experiments* lead him to the following conclusions :—

1. When the two currents (electric) are parallel, the intensities produced by each of them interfere with each other; when they are opposed they aid each other.

2. In the first case the sum of magnetisation developed is diminished; in the second it is increased.

3. If the theory of solenoids be admitted, the action of the parallel currents should be mutual and the sum of the intensities augmented; the inverse occurs.

4. When the currents of the bobbins are in opposite directions, they should act inversely on the particular currents of the iron and the effect be reduced; they aid, on the contrary.

* The experiments are made with a straight bar of 2 metres length, having a bobbin on each end.

5. The action of the bobbins should be nil at the middle of the bar, but it is not.

These phenomena M. Jamin considers fully to call for a modification of the theory of solenoids.

"*Comptes Rendus*," Tome LXXVIII. No. 3.

MEASUREMENT OF THE MAGNETIC MOMENT OF VERY SMALL MAGNETISED NEEDLES.

By M. E. Bouty.

Let us conceive a rigid support moveable around a vertical axis. Let us fix on this support (1) a horizontal needle of which the magnetic moment M is known; (2) the needle of which we wish to determine the magnetic moment x. The needles are placed one beneath the other in such fashion that their axes shall be rectangular and at such a distance that their reciprocal action shall not alter the distribution of the magnetism in either.

The system as formed takes, under the influence of terrestrial magnetism, a determined position of equilibrium, and such that the magnetic axis of the needle M makes with the plane of the magnetic meridian an angle α determined by the equation

$$x = \mathrm{M} \tan \alpha.$$

If the moment x is rather small with relation to M, the angle α would be determined with precision by the optical method of Gauss and Weber. To this end the support of the needles carries a little vertical silver mirror, in which can be observed with the aid of a telescope the image of a horizontal divided scale, placed above the objective lens and perpendicular to the optical axis of the telescope.

It is advantageous to make the moment M of the directive needle as feeble as may be desired. For by this process may be measured the magnetic moment of very small needles. I have effected, with sufficient precision, the measurement (relatively) of needles 1 to 2 m.m. in length and of 0·2 m.m. diameter.

When it is wished to inter-compare the magnetic moments x x' of several small needles it is not necessary to know the moment M of the directive needle. We have, in effect—

$$\frac{x'}{x} = \frac{\tan \alpha'}{\tan \alpha};$$

And, because of the minuteness of the angles α α'—

$$\frac{x'}{x} = \frac{\tan \alpha'}{\tan \alpha} = \frac{\tan \alpha'}{\tan \alpha} = \frac{\alpha'}{\alpha} = \frac{\tan 2\alpha'}{\tan 2\alpha} = \frac{n'}{n}.$$

Where n or n' are the two readings obtained on the scale, of which the zero is supposed in the plane of the magnetic meridian.

Such is the principle of the method employed. In practice the support of the needles is a fine rod of hard and difficultly fusible wax. The directive needle is fixed below the rod, and a fine glass tube traverses the rod at its superior part in such manner that its axis may be horizontal and perpendicular to the axis of the tube. The mirror is previously fastened parallel to the axis of the tube. The system is suspended by a small hook of copper wire and a cocoon-fibre. A disc of red-copper is placed beneath the directive needle to damp its oscillations. The position of the telescope and scale follows Weber's method.

It is to be remarked that when we introduce a non-magnetised needle in the axis of the tube we displace in general, by a very small quantity, the centre of gravity of the suspended system; but it is clear that this displacement is without influence on the readings.

Among the conditions of the apparatus may be one needing correction. The axis of the tube intended to receive the needles and the axis of the directive needle may not be exactly rectangular, but may make an angle $\frac{\pi}{2}-\beta$; because of the minuteness of β it will suffice to make two measures, turning the needle end for end in the tube; the mean of the two measures furnishes the exact value of the deviation.

It is also supposed in the preceding that the magnetic meridan is invariable, which is not true; the error resulting from variations in the declination is an appreciable quantity in the conditions under which the instrument is placed. If the variations are rapid and irregular, this cause of error may be eliminated by taking a third measure with the tube-needle direct. If the observations have been made at nearly equal intervals we may take the mean of measures 1 and 3, and the mean of these and of the second measure will not be sensibly affected by variation of the declination.

It is not necessary that the apparatus should be at rest to effect these observations; given that the oscillations have a sufficiently minute amplitude, we may observe the divisions of the scales corresponding to the commencement and to the end of an oscillation and to the end of the following one, and take the mean

$$\frac{\frac{n_1+n_2}{2}+n_3}{2}.$$

This operation, repeated several consecutive times, gives the position of equilibrium with much greater exactitude than a direct determination.

"*Comptes Rendus,*" Tome LXXVIII. No. 6.

SAFETY CABLE AGAINST FIRE.

By Messrs. Alph. Toly and P. Barbier.

The authors cause a cable to be made of an easily fusible insulating compound, containing in close juxtaposition two wires. To the ends of one of these wires a pole of the battery and a terminal of a trembling bell is attached; the other pole of the battery and terminal of the bell is attached to the ends of the remaining wire. A fire would melt the insulating compound, cause the wires to form contact, and the bell would ring.

No. 8.

ON THE PERMANENT MAGNETISM OF STEEL.

By M. E. Bouty.

The phrase, *coer*... *ce*, expresses but a vague comparison between the phe-
...na of magne... steel and of friction. This relation between two orders

of phenomena so complex is especially artificial, and absolutely excludes the facts relative to the temporary magnetisation of steel.

Wiedman has compared, with better fruit, the phenomena in question to those which depend from the elasticity of solid bodies, specially to the phenomena produced by torsion. Though it does not constitute, correctly speaking, a theory, this comparison has the advantage of expressing a real physical relation, since torsion modifies the magnetic state of a bar, and the modifications of this magnetic state, in their turn, modify the torsion previously existing in the bar.

It has long been known that a certain temporary magnetism can be superposed upon a permanent magnetism of contrary direction, and this can be made to reappear after the exterior force has ceased. For example, if we submit a bar of magnetised steel to the action of a current too feeble to entirely demagnetise it, we observe during the action of the current a diminution of the magnetism of the bar, which can be carried as far as a reversal of its poles, whilst after the cessation of the current we find the bar still to be magnetised in its primary direction.

The following fact, which I have observed, seems to me still more curious: I take a bundle of square section formed by the union of four square bars of the same length. This bundle is hard-tempered and immediately magnetised; finally it is dismounted and the magnetic moment of each bar measured individually. It is found that the sum of these moments is very notably superior to the magnetic moment of the bundle. Uniting the bars, two by two, the sum of the magnetic moments of the partial bundles is intermediate to the moment of the total bundle and of the separate bars. Finally, if we reconstitute the primitive bundle the magnetic moment resumes its first value.

In this experiment the bundle, that free from all anterior magnetisation has been submitted only once to the action of the magnetising helix, is, at the moment of the first separation, in an absolutely normal condition. In separating the bars we suppress, it is true, their reciprocal reaction, and we know that it acts in each of them in a direction contrary to that of the permanent magnetism; but this suppression should have effect only upon the temporary magnetism. Thus, even in a normal bar, a certain permanent magnetism is found superposed to a temporary magnetism of contrary direction.

It would seem, then, natural enough to return to the old hypothesis, according to which the condition, whatever it may be, that corresponds to the possibility of the conservation of a certain permanent magnetism, would only be communicated, in steeling or tempering, to a certain number of molecules, the others preserving their primary properties. If we remark (1) that the laws of the temporary magnetism of steel appear identical with the laws of induced magnetism in soft iron, that the development of permanent magnetism is eminently variable from one kind of iron or steel to another, and for the same kind, according to the physical conditions, sometimes insignificant, we shall be led to examine the consequences of this hypothesis.

Let us consider a cylinder, of elementary dimensions, whose length, relati... its diameter, is very great. Let us suppose two kinds of magnetic elements ... gated at hazard, but in determined proportion, in all parts of the cylinder, and a... upon a magnetic force F in the direction of the axis. If the ... molecules be depr... of the coercitive power alone existing, the cylinder would take a magnetic mom...

F Δv, where Δv represents the volume of the cylinder ... k a coefficient t... depends upon the density of the molecules considered. T... ...ules endowed wi... coercitive power, similarly, if they were alone, would ... magnetic mom... q F Δv.

If we suppose the coefficients of induction k and q constant, which is sensibly true for small values of the inductive forces, and if we designate by c a coefficient dependent upon the grouping of the magnetic elements of two kinds, we find that the magnetic moment communicated to the cylinder by the force F will be, taking account of the reactions of the two sorts of molecules,

$$M = \frac{k + q + 2ckq}{1 - c^3 kq} F \Delta \nu;$$

after the cessation of the force F they will preserve a moment

$$m = q \frac{(1 - ck)^2}{(1 - c^2 k q)} F \Delta \nu.$$

This quantity represents what is ordinarily termed *permanent* magnetism. The temporary magnetism which disappears by the cessation of the force F is—

$$\mu = M - m = K F \Delta \nu.$$

We see by this that the two coefficients of temporary and permanent magnetism are not quantities of the same kind. The quantity q, analogous by its action to k, is obtained by dividing the ordinary coefficient of permanent magnetism by $\frac{(1 + c k)^2}{1 - c^2 k q}$.

It is evident that the total magnetic moment M is intermediate to those that will produce the same force F, acting on two cylinders equal to the first, and composed of only one kind of molecules with the same total density; but this is not the case, for the residual moment m, which, for a given value of q, is as much greater as the coefficient k of temporary magnetism is itself greater, and as the coefficient k relative to soft iron is enormous, we see that the adjunction of a certain quantity of soft iron to the harder steel may augment its residual magnetism. This may account for the augmentation of the power of bundles of steel magnets by armatures of soft iron.

PAGET HIGGS, LL.D., D.Sc.

NOVEL APPLICATION OF TELEGRAPH WIRE.

Mr. L. Schwendler showed at the meeting of the Asiatic Society a crow's nest made of pieces of telegraph wire twisted together in a most ingenious and knowing fashion. He said that lately such nests had been found frequently, and that it seemed as if the crows of India benefited by the introduction of Western civilisation, and were by no means behind the age. As long as the crows built their wire nests on trees and buildings only, he, as a telegraph engineer, would not object, but often they selected telegraph posts, between which and the telegraph wires they built these wire nests, causing what are known as "earth" and "contact," and interfering with communication. Crows, however, were by no means the only animals interfering by their domestic arrangements with overland telegraphy. Wasps built their mud nests in the porcelain insulators, causing, in rain and dew, leakage from the iron to the ground. Birds of prey frequently dropped dead fish and other offal on the wires, causing contact. These were all frequent sources of temporary interference with telegraphic communication on overland lines, and they, combined with many other facts not necessary to mention, seemed to show that it would be a very great advantage to use subterranean telegraphs instead of overland lines.—*Calcutta Englishman.*

INDEX.